



# The Psychological Review.

THE REVIEW, having entered upon its second volume, ventures to call attention to the following partial list of American University Professors and Instructors who have written for it. Their articles comprise all departments of psychological work:—

## Harvard:

WM. JAMES.  
HUGO MUNSTERBERG.  
JOSEPH ROYCE.  
GEO. SANTAYANA.  
C. W. FRYE.  
HERBERT NICHOLS.  
EDGAR PIERCE.

## Yale:

GEO. T. LADD.  
G. M. DUNCAN.  
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G. S. FULLERTON.  
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JOHN DEWEY.  
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## Columbia:

N. M. BUTLER.  
J. MCK. CATTELL.  
M. A. STARR.  
H. F. OSGOOD.  
J. H. HENLOP.  
LIVINGSTON FARRAND.

## Princeton:

A. T. OSGOOD.  
J. MARK BALDWIN.  
J. G. HIRSH.  
H. C. WARREN.

Other University teachers have contributed from Michigan, Wisconsin, Clark, Cornell, Wellesley, Brown, Wesleyan, Indiana, Iowa, Lake Forest, Stanford, Bryn Mawr, Smith, Illinois, Bellevue Medical, and the Catholic University of Washington. Foreign writers for the REVIEW have included:

CARL STUMPF, Bonn; JAMES SULLY, London; A. BINET, Paris; J. PHILIPPE, Paris; A. KIRCHMANN, Toronto; F. KINNDY, Leipzig.

The first issue of an annual Bibliography of Psychological Literature called *The Psychological Index* is announced. It comprises titles of the literature of Psychology and cognate subjects in all languages for 1894. It may be ordered from Macmillan & Co., price 75 cents, or, when ordered by a subscriber to the *Psychological Review*, 50 cents. The *Index* is prepared by Dr. Livingston Farrand of Columbia College, and Mr. H. C. Warren of Princeton, in cooperation with the editors of the *Review*.

The *Review* also announces the founding of a series of *Monograph Supplements*, consisting of longer dissertations, researches, &c., to be issued at intervals as they may be accepted by the editors. They will be numbered consecutively and 1-6 numbers will constitute a volume. The first of these *Monograph Supplements* is now ready. It is a research on *Sensations from Pressure and Impact* by Dr. Harold Griffing.

These *Monographs* may be ordered from the same publishers at the rate (by subscription) of \$1 a volume of about 600 pages; or by single numbers, at from 50 cents to \$1.00, according to the number of pages. Papers for publication in this series during 1895 may be sent to Prof. J. Mark Baldwin, Princeton, N. J.

# THE PSYCHOLOGICAL REVIEW.

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## THE KNOWING OF THINGS TOGETHER.<sup>1</sup>

BY PROFESSOR WILLIAM JAMES,

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### I.

The nature of the synthetic unity of consciousness is one of those great underlying problems that divide the psychological schools. We know, say, a dozen things singly through a dozen different mental states. But on another occasion we may know the same dozen things together through a single mental state. The problem is as to the relation of the previous many states to the later one state. In physical nature, it is universally agreed, a multitude of facts always remain the multitude they were and appear as one fact only when a mind comes upon the scene and so views them, as when H-O-H appear as 'water' to a human spectator. But when, instead of extramental 'things,' the mind combines its own 'contents' into a unity, what happens is much less plain.

The matters of fact that give the trouble are among our most familiar experiences. We know a lot of friends and can think of each one singly. But we can also think of them together, as composing a 'party' at our house. We can see single stars appearing in succession between the clouds on a stormy night, but we can also see whole constellations of those stars at once when the wind has blown the clouds away. In a glass of lemonade we can taste both

<sup>1</sup> Read as the President's Address before the American Psychological Association at Princeton, December, 1894, and reprinted with some unimportant omissions, a few slight revisions, and the addition of some explanatory notes.

the lemon and the sugar at once. In a major chord our ear can single out the *c*, *e*, *g*, and *c'*, if it has once become acquainted with these notes apart. And so on through the whole field of our experience, whether conceptual or sensible. Neither common sense nor commonplace psychology finds anything special to explain in these facts. Common sense simply says the mind 'brings the things together,' and common psychology says the 'ideas' of the various things 'combine,' and at most will admit that the occasions on which ideas combine may be made the subject of inquiry. But to formulate the phenomenon of knowing things together thus as a combining of ideas, is already to foist in a theory about the phenomenon simply. Not so should a question be approached. The phenomenon offers itself, in the first instance, as that of *knowing things together*; and it is in those terms that its solution must, in the first instance at least, be sought.

'Things,' then; to 'know' things; and to know the 'same' things 'together' which elsewhere we knew singly—here, indeed, are terms concerning each of which we must put the question, 'What do we *mean* by it when we use it?'—that question that Shadworth Hodgson lays so much stress on, and that is so well taught to students, as the beginning of all sound method, by our colleague Fullerton. And in exactly ascertaining what we do mean by such terms there might lie a lifetime of occupation.

For we do mean something; and we mean something true. Our terms, whatever confusion they may connote, denote at least a fundamental fact of our experience, whose existence no one here present will deny.

## II.

What, then, do we mean by 'things'? To this question I can only make the answer of the idealistic philosophy. For the philosophy that began with Berkeley, and has led up in our tongue to Shadworth Hodgson, things have no other nature than thoughts have, and we know of no things that are not given to somebody's experience. When I see

the thing white paper before my eyes, the nature of the thing and the nature of my sensations are one. Even if with science we supposed a molecular architecture beneath the smooth whiteness of the paper, that architecture itself could only be defined as the stuff of a farther possible experience, a vision, say, of certain vibrating particles with which our acquaintance with the paper would terminate if it were prolonged by magnifying artifices not yet known. A thing may be my phenomenon or some one else's; it may be frequently or infrequently experienced; it may be shared by all of us; one of our copies of it may be regarded as the original, and the other copies as representatives of that original; it may appear very differently at different times; but whatever it be, the stuff of which it is made is thought-stuff, and whenever we speak of a thing that is out of our own mind, we either mean nothing; or we mean a thing that was or will be in our own mind on another occasion; or, finally, we mean a thing in the mind of some other possible receiver of experiences like ours.

Such being 'things,' what do we mean by saying that we 'know' them?

There are two ways of knowing things, knowing them immediately or intuitively, and knowing them conceptually or representatively. Although such things as the white paper before our eyes can be known intuitively, most of the things we know, the tigers now in India, for example, or the scholastic system of philosophy, are known only representatively or symbolically.

Suppose, to fix our ideas, that we take first a case of conceptual knowledge; and let it be our knowledge of the tigers in India, as we sit here. Exactly what do we *mean* by saying that we here know the tigers? What is the precise fact that the cognition so confidently claimed is *known-as*, to use Shadworth Hodgson's inelegant but valuable form of words?

Most men would answer that what we mean by knowing the tigers is having them, however absent in body, become in some way present to our thought; or that our knowledge of them is known as presence of our thought to them. A great mystery is usually made of this peculiar presence in

absence; and the scholastic philosophy, which is only common sense grown pedantic, would explain it as a peculiar kind of existence, called *intentional inexistence*, of the tigers in our mind. At the very least, people would say that what we mean by knowing the tigers is mentally *pointing* towards them as we sit here.

But now what do we mean by *pointing*, in such a case as this? What is the pointing known-as, here?

To this question I shall have to give a very prosaic answer—one that traverses the prepossessions not only of common sense and scholasticism, but also those of nearly all the epistemological writers whom I have ever read. The answer, made brief, is this: The pointing of our thought to the tigers is known simply and solely as a procession of mental associates and motor consequences that follow on the thought, and that would lead harmoniously, if followed out, into some ideal or real context, or even into the immediate presence, of the tigers. It is known as our rejection of a jaguar, if that beast were shown us as a tiger; as our assent to a genuine tiger if so shown. It is known as our ability to utter all sorts of propositions which don't contradict other propositions that are true of the real tigers. It is even known, if we take the tigers very seriously, as actions of ours which may terminate in directly intuited tigers, as they would if we took a voyage to India for the purpose of tiger-hunting and brought back a lot of skins of the striped rascals which we had laid low. In all this there is no self-transcendancy in our mental images taken by themselves. They are one physical fact; the tigers are another; and their pointing to the tigers is a perfectly commonplace physical relation, if you once grant a connecting world to be there. In short, the ideas and the tigers are in themselves as loose and separate, to use Hume's language, as any two things can be; and pointing means here an operation as external and adventitious as any that nature yields.<sup>1</sup>

<sup>1</sup> A stone in one field may 'fit,' we say, a hole in another field. But the relation of 'fitting,' so long as no one carries the stone to the hole and drops it in, is only one name for the fact that such an act may happen. Similarly with the knowing of the tigers here and now. It is only an anticipatory name for a further associative and terminative process that may occur.

I hope you may agree with me now that in representative knowledge there is no special inner mystery, but only an outer chain of physical or mental intermediaries connecting thought and thing. *To know an object is here to lead to it through a context which the world supplies.* All this was most instructively set forth by our colleague Miller, of Bryn Mawr, at our meeting in New York last Christmas, and for re-confirming my sometime wavering opinion, I owe him this acknowledgment.<sup>1</sup>

Let us next pass on to the case of immediate or intuitive acquaintance with an object, and let the object be the white paper before our eyes. The thought-stuff and the thing-stuff are here indistinguishably the same in nature, as we saw a moment since, and there is no context of intermediaries or associates to stand between and separate the thought and thing. There is no 'presence in absence' here, and no 'pointing,' but rather an allround embracing of the paper by the thought; and it is clear that the knowing cannot now be explained exactly as it was when the tigers were its object. Dotted all through our experience are states of immediate acquaintance just like this. Somewhere our belief always does rest on ultimate data like the whiteness, smoothness, or squareness of this paper. Whether such qualities be truly ultimate aspects of being or only provisional suppositions of ours, held-to till we get better informed, is quite immaterial for our present inquiry. So long as it is believed in, we see our object face to face. What now do we mean by 'knowing' such a sort of object as this? For this is also the way in which we should know the tiger if our conceptual idea of him were to terminate by having led us to his lair?

This address must not become too long, so I must give my answer in the fewest words. And let me first say this: So far as the white paper or other ultimate datum of our experience is considered to enter also into some one else's experience, and we, in knowing it, are held to know it there as well as here; so far again as it is considered to be a mere

<sup>1</sup> See also Dr. Miller's article on Truth and Error, in the *Philosophical Review*, July, 1893.

mask for hidden molecules that other now impossible experiences of our own might some day lay bare to view; so far it is a case of tigers in India again—the things known being absent experiences, the knowing can only consist in passing smoothly towards them through the intermediary context that the world supplies. But if our own private vision of the paper be considered in abstraction from every other event, as if it constituted by itself the universe (and it might perfectly well do so, for aught we can understand to the contrary), then the paper seen and the seeing of it are only two names for one indivisible fact which, properly named, is *the datum, the phenomenon, or the experience*. The paper is in the mind and the mind is around the paper, because paper and mind are only two names that are given later to the one experience, when, taken in a larger world of which it forms a part, its connections are traced in different directions.<sup>1</sup> *To know immediately, then, or intuitively, is for mental content and object to be identical.* This is a very different definition from that which we gave of representative knowledge; but neither definition involves those mysterious notions of self-transcendency and presence in absence which are such essential parts of the ideas of knowledge, both of common men and of

<sup>1</sup> What is meant by this is that 'the experience' can be referred to either of two great associative systems, that of the experiencer's mental history, or that of the experienced facts of the world. Of both of these systems it forms part, and may be regarded, indeed, as one of their points of intersection. One might let a vertical line



stand for the mental history; but the same object, O, appears also in the mental history of different persons, represented by the other vertical lines. It thus ceases to be the private property of one experience, and becomes, so to speak, a shared or public thing. We can track its outer history in this way, and represent it by the horizontal line. [It is also known representatively at other points of the vertical lines, or intuitively there again, so that the line of its outer history would have to be looped and wandering, but I make it straight for simplicity's sake.] In any case, however, it is the same *stuff* that figures in all the sets of lines.

philosophers. Is there no experience that can justify these notions, and show us somewhere their original?

I think the mystery of presence in absence (though we fail to find it between one experience and another remote experience to which it points, or between the 'content' and 'object' of any one experience falsely rent asunder by the application to it of these two separate names) may yet be found, and found between the parts of a single experience. Let us look for it, accordingly, in its simplest possible form. What is the smallest experience in which the mystery remains? If we seek, we find that there is no datum so small as not to show the mystery. The smallest effective pulse of consciousness, whatever else it may be consciousness of, is also consciousness of passing time. The tiniest feeling that we can possibly have involves for future reflection two sub-feelings, one earlier and the other later, and a sense of their continuous procession. All this has been admirably set forth by Mr. Shadworth Hodgson,<sup>1</sup> who shows that there is literally no such datum as that of the present moment, and no such content, and no such object, except as an unreal postulate of abstract thought. The *passing* moment is the only thing that ever concretely was or is or shall be; and in the phenomenon of elementary memory, whose function is to apprehend it, earlier and later are present to each other in an experience that feels either only on condition of feeling both together.

We have the same knowing together in the matter that fills the time. The rush of our thought forward through its fringes is the everlasting peculiarity of its life. We realize this life as something always off its balance, something in transition, something that shoots out of a darkness through a dawn into a brightness that we know to be the dawn fulfilled. In the very midst of the alteration our experience comes as one continuous fact. 'Yes,' we say at the moment of full brightness, *this* is what I meant. No, we feel at the moment of the dawning, this is not yet the meaning, there is more to come. In every crescendo of sensation, in every effort

<sup>1</sup> *Philosophy of Reflection*, Vol. I, p. 248 ff.

to recall, in every progress towards the satisfaction of desire, this succession of an emptiness and fulness that have reference to each other and are one flesh is the essence of the phenomenon. In every hindrance of desire the sense of ideal presence of what is absent in fact, of an absent, in a word, which the only function of the present is to *mean*, is even more notoriously there. And in the movement of thoughts not ordinarily classed as involving desire, we have the same phenomenon. When I say *Socrates is mortal*, the moment *Socrates* is incomplete; it falls forward through the *is* which is pure movement, into the *mortal*, which is indeed bare mortal on the tongue, but for the mind, is *that mortal*, the *mortal Socrates*, at last satisfactorily disposed of and told off.

Here, then, inside of the minimal pulse of experience which, taken as object, is change of feeling, and, taken as content, is feeling of change, is realized that absolute and essential self-transcendancy which we swept away as an illusion when we sought it between a content taken as a whole and a supposed objective thing outside. *Here in the elementary datum of which both our physical and our mental worlds are built, we find included both the original of presence in absence and the prototype of that operation of knowing many things together which it is our business to discuss.*<sup>1</sup>

<sup>1</sup> It seems to me that we have here something like what comes before us in the psychology of space and time. Our original intuition of space is the single field of view; our original intuition of time covers but a few seconds; yet by an ideal piecing together and construction we frame the notions of immensity and eternity, and suppose dated events and located things therein, of whose actual intervals we grasp no distinct idea. So in the case before us. The way in which the constituents of one undivided datum drag each other in and run into one, saying *this* is what *that* means, gives us our original intuition of what knowing is. That intuition we extend and constructively build up into the notion of a vast tissue of knowledge, shed along from experience to experience until, dropping the intermediary data from our thought, we assume that terms the most remote still know each other, just after the fashion of the parts of the prototypal fact. Cognition here is only constructive, as we have already seen. But he who should say, arguing from its nature here, that it nowhere is direct, and seek to construct it without an originally given pattern, would be like those psychologists who profess to develop our idea of space out of the association of data that possess no original extensity. Grant the *sort* of thing that is meant by presence in absence, by self-transcendancy, by reference to another, by pointing forward or back, by knowledge in short, somewhere in our experience, be it in ever so small a corner, and the construction of pseudo-cases elsewhere follows as a matter of course. But to get along without the real thing *anywhere* seems difficult indeed.

For the fact that past and future are already parts of the least experience that can really be, is just like what we find in any other case of an experience whose parts are many. Most of these experiences are of objects perceived to be simultaneous and not to be immediately successive as in the heretofore considered case. The field of view, the chord of music, the glass of lemonade are examples. But the gist of the matter is the same—it is always knowing-together. You cannot separate the consciousness of one part from that of all the rest. What is given is pooled and mutual; there is no dark spot, no point of ignorance; no one fraction is eclipsed from any other's point of view. Can we account for such a being-known-together of complex facts like these?

The general *nature* of it we can probably never account for, or tell how such a unity in manyness can be, for it seems to be the ultimate essence of all experience, and anything less than it apparently cannot be at all. But the particular *conditions* whereby we know particular things together might conceivably be traced, and to that humble task I beg leave to devote the time that remains.

### III.

Let me say forthwith that I have no pretension to give any positive solution. My sole ambition now is, by a little classification, to smooth the ground somewhat so that some of you, more able than I, may be helped to advance, before our next meeting perhaps, to results that I cannot obtain.

Now, the first thing that strikes us in these complex cases is that the condition by which one thing may come to be known together with other things is an *event*. It is often an event of the purely physical order. A man walks suddenly into my field of view, and forthwith becomes part of it. I put a drop of cologne-water on my tongue, and, holding my nostrils, get the taste of it alone, but when I open my nostrils I get the smell together with the taste in mutual suffusion. Here it would seem as if a sufficient condition of the knowing of (say) three things together were the fact that the three several physical conditions of the

knowing of each of them were realized at once. But in many other cases we find on the contrary that the physical conditions are realized without the things being known together at all. When absorbed in experiments with the cologne-water, for example, the clock may strike, and I not know that it has struck. But again, some seconds after the striking has elapsed, I may, by a certain shifting of what we call my attention, hark back to it and resuscitate the sound, and even count the strokes in memory. The condition of knowing the clock's striking is here an event of the mental order which must be added to the physical event of the striking before I can know it and the cologne-water at once. Just so in the field of view I may entirely overlook and fail to notice even so important an object as a man, until the inward event of altering my attention makes me suddenly see him with the other objects there. In those curious phenomena of dissociation of consciousness with which recent studies of hypnotic, hysteric and trance-states have made us familiar (phenomena which surely throw more new light on human nature than the work of all the psycho-physical laboratories put together), the event of hearing a 'suggestion,' or the event of passing into trance or out of it, is what decides whether a human figure shall appear in the field of view or disappear, and whether a whole set of memories shall come before the mind together, along with its other objects, or be excluded from their company. There is in fact no possible object, however completely fulfilled may be the outer condition of its perception, whose entrance into a given field of consciousness does not depend on the additional inner event called attention.

Now, it seems to me that this need of a final inner event, over and above the mere sensorial conditions, quite refutes and disposes of the associationist theory of the unity of consciousness. By associationist theory, I mean any theory that says, either implicitly or explicitly, that for a lot of objects to be known together, it suffices that a lot of conscious states, each with one of them as its content, should exist, as James Mill says, 'synchronically.' Synchronical existence of the ideas does not suffice, as the facts we now

have abundantly show. Gurney's, Binet's and Janet's proofs of several dissociated consciousnesses existing synchronically, and dividing the subject's field of knowledge between them, is the best possible refutation of any such view.

Union in consciousness must be *made* by something, must be brought about; and to have perceived this truth is the great merit of the anti-associationist psychologists.<sup>1</sup> The form of unity, they have obstinately said, must be specially accounted for; and the form of unity the radical associationists have as obstinately shied away from and ignored, though their accounts of those preliminary conditions that supply the matters to be united have never been surpassed. As far as these go, we are all, I trust, associationists, and reverers of the names of Hartley, Mill, and Bain.

Let us now rapidly review the chief attempts of the anti-associationists to fill the gap they discern so well in the associationist tale.

1. *Attention*.—Attention, we say, by turning to an object, includes it with the rest; and the naming of this faculty in action has by some writers been considered a sufficient account of the decisive 'event.'<sup>2</sup> But it is plain that the act of Attention itself needs a farther account to be given, and such an account is what other theories of the event implicitly give.

We find four main types<sup>3</sup> of other theory of how par-

<sup>1</sup> In this rapid paper I content myself with arguing from the experimental fact that something *happens* over and above the realization of sensorial conditions, wherever an object adds itself to others already 'before the mind.' I say nothing of the logical self-contradiction involved in the associationist doctrine that the two facts, 'A is known,' and 'B is known,' are the third fact, 'A + B are known together.' Those whom the criticisms already extant in print of this strange belief have failed to convince, would not be persuaded, even though one rose from the dead. The appeal to the actual facts of dissociation may make impression, however, even on such hardened hearts as theirs.

<sup>2</sup> It might seem natural to mention Wundt's doctrine of 'Apperception' here. But I must confess my inability to say anything about it that would not resolve itself into a tedious comparison of texts. Being alternately described as intellection, will, feeling, synthesis, analysis, principle and result, it is too 'protean' a function to lend itself to any simplified account at second hand.

<sup>3</sup> It is only for the sake of completeness that we need mention such notions of a sort of mechanical and chemical activity between the ideas as we find in Herbart,

ticular things get known together, a physiological, a psychological, an animistic, and a transcendentalist type. Of the physiological or 'psycho-physical' type many varieties are possible, but it must be observed that none of them pretends to assign anything more than an empirical law. A psycho-physical theory can couple certain antecedent conditions with their result; but an explanation, in the sense of an inner reason why the result should have the nature of one content with many parts instead of some entirely different nature, is what a psycho-physical theory cannot give.<sup>1</sup>

2. *Reminiscence*.—Now, empirically, we have learned that things must be known in succession and singly before they can be known together.<sup>2</sup> If A, B, and C, for example, were outer things that came for the first time and affected our senses all at once, we should get one content from the lot of them and make no discriminations. The content would symbolically point to the objects A, B, C, and eventually terminate there, but would contain no parts that were immediately apprehended as standing for A, B, and C severally. Let A, B, and C stand for pigments, or for a tone and its overtones, and you will see what I mean when I say that the first result on consciousness of their falling together on the eye or ear would be a single new

Steinthal and others. These authors see clearly that mere synchronical existence is not combination, and attribute to the ideas dynamic influences upon each other; pressures and resistances according to Herbart, and according to Steinthal 'psychic attractions.' But the philosophical foundation of such physical theories have been so slightly discussed by their authors that it is better to treat them only as rhetorical metaphors and pass on. Herbart, moreover, must also be mentioned later, along with the animistic writers.

<sup>1</sup> We find this impotence already when we seek the conditions of the passing pulse of consciousness, which, as we saw, always involves time and change. We account for the passing pulse, physiologically, by the overlapping of dying and dawning brain-processes; and at first sight the elements time and change, involved in both the brain-processes and their mental result, gives a similarity that, we feel, might be the real reason for the psycho-physic coupling. But the moment we ask 'metaphysical' questions—"Why not each brain-process felt apart?—Why just this amount of time, neither more nor less?" etc., etc.—we find ourselves falling back on the empirical view as the only safe one to defend.

<sup>2</sup> The latest empirical contribution to this subject, with which I am acquainted, is Dr. Herbert Nichols' excellent little monograph, 'Our Notions of Number and Space.' Boston, Ginn & Co., 1894.

kind of feeling rather than a feeling with three kinds of inner part. Such a result has been ascribed to a 'fusion' of the three feelings of A, B, and C; but there seems no ground for supposing that, under the conditions assumed, these distinct feelings have ever been aroused at all. I should call the phenomenon one of *indiscriminate knowing together*, for the most we can say under the circumstances is that the content resembles somewhat each of the objects A, B, and C, and knows them each potentially, knows them, that is, by possibly leading to each smoothly hereafter, as we know Indian tigers even whilst sitting in this room.

But if our memory possess stored-up images of former A-s, B-s, and C-s, experienced in isolation, we get an altogether different content, namely, one through which we know A, B, and C together, and yet know each of them in discrimination through one of the content's own parts. This has been called a 'colligation' or *Verknüpfung* of the 'ideas' of A, B, and C, to distinguish it from the afore-said fusion. Whatever we may call it, we see that its physiological condition is more complex than in the previous case. In both cases the outer objects, A, B, and C, exert their effects on the sensorium. But in this case there is a coöperation of higher tracts of memory which in the former case was absent. *Discriminative knowing-together, in short, involves higher processes of reminiscence.* Do these give the element of manyness, whilst the lower sensorial processes that by themselves would result in mere 'fusion,' give the unity to the experience? The suggestion is one that might repay investigation, although it has against it two pretty solid objections: first, that in man the consciousness attached to infra-cortical centres is altogether subliminal, if it exist; and, second, that in the cortex itself we have not yet discriminated sensorial from ideational processes. Possibly the frontal lobes, in which Wundt has supposed an *Apperceptionsorgan*, might serve a turn here. In any case it is certain that, into our present rough notions of the cortical functions, the future will have to weave distinctions at present unknown.

3. *Synergy*.—The theory that, physiologically, the oneness precedes the manyness, may be contrasted with a theory that our colleagues Baldwin and Münsterberg are at present working out, and which places the condition of union of many data into one datum, in the fact that the many pour themselves into one motor discharge. The motor discharge being the last thing to happen, the condition of manyness would physiologically here precede and that of oneness follow. A printed word is apprehended as one object, at the same time that each letter in it is apprehended as one of its parts. Our secretary, Cattell, long ago discovered that we recognize words of four or five letters by the eye as quickly, or even more quickly, than we recognize single letters. Recognition means here the motor process of articulation; and the quickness comes from the fact that all the letters in the particular combination unhesitatingly coöperate in the one articulatory act. I suppose such facts as these to lie at the base of our colleagues' theories, which probably differ in detail, and which it would be manifestly unjust to discuss or guess about in advance of their completer publication. Let me only say that I hope the latter may not be long delayed.

These are the only types of physiological theory worthy of mention. I may next pass to what, for brevity's sake, may be called *psychological accounts* of the event that lets an object into consciousness, or, by not occurring, leaves it out. These accounts start from the fact that what figures as part of a larger object is often perceived to have relations to the other parts. Accordingly the event in question is described as an *act of relating thought*. It takes two forms.

4. *Relating to Self*.—Some authors say that nothing can enter consciousness except on condition that it be related to the self. Not *object*, but *object-plus-me*, is the minimum knowable.

5. *Relating to other Objects*.—Others think it enough if the incoming object be related to the other objects already there. To fail to appear related is to fail to be known at all. To appear related is to appear with other objects. If relations were correlates of special cerebral processes, the

addition of these to the sensorial processes would be the wished-for event. But brain physiology as yet knows nothing of such special processes, so I have called this explanation purely psychological. There seem to be fatal objections to it as a universal statement, for the reference to self, if it exist, must in a host of cases be altogether subconscious; and introspection assures us that in many half-waking and half-drunken states the relations between things that we perceive together may be of the dimmest and most indefinable kind.

6. *The Individual Soul.*—So we next proceed to the *animistic account*. By this term I mean to cover every sort of individualistic soul-theory. I will say nothing of older opinions; but in modern times we have two views of the way in which the union of a many by a soul occurs. For Herbart, for example, it occurs because the soul itself *is* unity, and all its *Selbsterhaltungen* are obliged to necessarily share this form. For our colleague Ladd, on the other hand, to take the best recent example, it occurs because the soul, which *is* a real unity indeed, furthermore performs a unifying *act* on the naturally separate data of sense—an act, moreover, for which no psycho-physical analogon can be found. It must be admitted that much of the reigning bias against the soul in so-called scientific circles is an unintelligent prejudice, traceable far more to a vague impression that it is a theological superstition than to exact logical grounds. The soul is an ‘entity,’ and, indeed, that worst sort of entity, a ‘scholastic entity;’ and, moreover, it is something to be damned or saved; so let’s have no more of it! I am free to confess that in my own case the antipathy to the Soul with which I find myself burdened is an ancient hardness of heart of which I can frame no fully satisfactory account even to myself. I passively agree that if there were Souls that we could use as principles of explanation, the *formal* settlement of the questions now before us could run far more smoothly towards its end. I admit that a soul is a medium of union, and that brain-processes and ideas, be they never so ‘synchronical,’ leave all mediating agency out. Yet, in spite of these concessions, I never find

myself actively taking up the soul, so to speak, and making it do work in my psychologizing. I speak of myself here because I am one amongst many, and probably few of us can give adequate reasons for our dislike. The more honor to our colleague from Yale, then, that he remains so unequivocally faithful to this unpopular principle! And let us hope that his forthcoming book may sweep what is blind in our hostility away.<sup>1</sup>

But all is not blind in our hostility. When, for example, you say that A, B, and C, which are distinct contents on other occasions, are now on this occasion joined into the compound content ABC by a unifying act of the soul, you say little more than that now they *are* united, unless you give some hint as to *how* the soul unites them. When, for example, the hysteric women which Pierre Janet has studied with such loving care, go to pieces mentally, and their souls are unable any longer to connect the data of their experience together, though these data remain severally conscious in dissociation, what is the condition on which this inability of the soul depends? Is it an impotence in the soul itself? or is it an impotence in the physiological conditions, which fail to stimulate the soul sufficiently to its synthetic task? The *how* supposes on the Soul's part a constitution adequate to the act. An hypothesis, we are told in the logic-books, ought to propose a being that has some other constitution and definition than that of barely performing the phenomenon it is evoked to explain. When physicists propose the 'ether,' for example, they propose it with a lot of incidental properties. But the soul proposed to us has no special properties or constitution of which we are informed. Nev-

<sup>1</sup> I ought, perhaps, to apologize for not expunging from my printed text these references to Professor Ladd, which were based on the impression left on my mind by the termination of his *Physiological Psychology*. It would now appear from the paper read by him at the Princeton meeting, and his '*Philosophy of Mind*,' just published, that he disbelieves in the Soul of old-fashioned ontology; and on looking again at the P. P., I see that I may well have misinterpreted his deeper meaning there. I incline to suspect, however, that he had himself not fully disentangled it when that work was written; and that between now and then his thought has been evolving somewhat, as Lotze's did, between his '*Medical Psychology*' and his '*Metaphysic*.' It is gratifying to note these converging tendencies in different philosophers; but I leave the text as I read it at Princeton, as a mark of what one could say not so very unnaturally at that date.

ertheless, since particular conditions do determine its activity, it must have a constitution of some sort. In either case, we ought to know the facts. But the soul-doctrine, as hitherto professed, not only doesn't answer such questions, it doesn't even ask them; and it must be radically rejuvenated if it expects to be greeted again as a useful principle in psychological philosophy. Here is work for our spiritualist colleagues, not only for the coming year, but for the rest of their lives.<sup>1</sup>

7. *The World-soul*.—The second spiritualist theory may be named as that of *transcendentalism*. I take it typically and not as set forth by any single author. Transcendentalism explains things by an over-soul of which all separate souls, sensations, thoughts, and data generally are parts. To be, as it would be known together with everything else in the world by this over-soul, is for transcendentalism the *true* condition of each single thing, and to pass into this condition is for things to fulfill their vocation. Such being known together, since it is the innermost reality of life, cannot on transcendentalist principles be explained or accounted for as a work wrought on a previous sort of reality. The monadic soul-theory starts with separate sensational data, and must show how they are *made* one. The transcendentalist theory has rather for its task to show how, being one, they can spuriously and illusorily be made to appear separate. The problem for the monadic soul, in short, is that of unification, and the problem for the over-soul is that of insulation. The removal of insulating obstruc-

<sup>1</sup>The soul can be taken in three ways as a unifying principle. An already existing lot of animated sensations (or other psychic data) may be simply *woven* into one by it; in which case the form of unity is the soul's only contribution, and the original stuff of the Many remains in the One as its stuff also. Or, secondly, the resultant synthetic One may be regarded as an immanent *reaction* of the Soul on the preëxisting psychic Many; and in this case the Soul, in addition to creating the new form, reproduces in itself the old stuff of the Many, superseding it for our use, and making it for us become subliminal, but not suppressing its existence. Or, thirdly, the One may again be the Soul's immanent reaction on a physiological, not on a mental, Many. In this case preëxisting *sensations or ideas* would not be there at all, to be either woven together or superseded. The synthetic One would be a primal psychic datum with parts, either of which might know the same object that a possible sensation, realized under other physiological conditions, could also know.

tions would sufficiently account for things reverting to their natural place in the over-soul and being known together. The most natural insulating or individualizing principle to invoke is the bodily organism. As the pipes of an organ let the pressing mass of air escape only in single notes, so do our brains, the organ pipes of the infinite, keep back everything but the slender threads of truth to which they may be pervious. As they obstruct more, the insulation increases, as they obstruct less it disappears. Now transcendental philosophers have as a rule not done much dabbling in psychology. But one sees no abstract reason why they might not go into psychology as fully as any one, and erect a psychophysical science of the conditions of more separate and less separate cognition which would include all the facts that psycho-physicists in general might discover. And they would have the advantage over other psycho-physicists of not needing to explain the nature of the resultant knowing-together when it should occur, for they could say that they simply begged it as the ultimate nature of the world.

This is as broad a disjunction as I can make of the different ways in which men have considered the conditions of our knowing things together. You will agree with me that I have brought no new insight to the subject, and that I have only gossiped to while away this unlucky presidential hour to which the constellations doomed me at my birth. But since gossip we have had to have, let me make the hour more gossipy still by saying a final word about the position taken up in my own *Principles of Psychology* on the general question before us, a position which, as you doubtless remember, was so vigorously attacked by our colleague from the University of Pennsylvania at our meeting in New York a year ago.<sup>1</sup> That position consisted in this, that I proposed to simply eliminate from psychology 'considered as a natural science' the whole business of ascertaining *how* we come to know things together or to know them at all. Such considerations, I said, should fall to metaphysics. That we do know

<sup>1</sup> Printed as an article entitled 'The Psychological Standpoint,' in this REVIEW, Vol. I, p. 113. (March, 1894.)

things, sometimes singly and sometimes together, is a fact. That states of consciousness are the vehicle of the knowledge, and depend on brain states, are two other facts. And I thought that a natural science of psychology might legitimately confine itself to tracing the functional variations of these three sorts of fact, and ascertaining and tracing what determinate bodily states are the condition when the states of mind know determinate things and groups of things. Most states of mind can be designated only by naming what objects they are 'thoughts-of,' *i. e.*, what things they know.

Most of those which know compound things are utterly unique and solitary mental entities demonstrably different from any collection of simpler states to which the same objects might be singly known.<sup>1</sup> Treat them all as unique in entity, I said then; let their complexity reside in their plural cognitive function; and you have a psychology which, if it doesn't ultimately explain the facts, also does not, in ex-

<sup>1</sup> When they know conceptually they don't even remotely resemble the simpler states. When they know intuitively they resemble, sometimes closely, sometimes distantly, the simpler states. The sour and sweet in lemonade are extremely unlike the sour and sweet of lemon juice and sugar, singly taken, yet like enough for us to 'recognize' these 'objects' in the compound taste. The several objective 'notes' recognized in the chord sound differently and peculiarly there. In a motley field of view successive and simultaneous contrast give to each several tint a different hue and luminosity from that of the 'real' color into which it turns when viewed without its neighbors by a rested eye. The difference is sometimes so slight, however, that we overlook the 'representative' character of each of the parts of a complex content, and speak as if the latter were a cluster of the original 'intuitive' states of mind that, occurring singly, know the 'object's' several parts in separation. Prof. Meinong, for example, even after the true state of things had been admirably set forth by Herr H. Cornelius (in the *Vierteljahrschrift f. wiss. Phil.*, XVI, 404; XVII, 30), returns to the defence of the radical associationist view (in the *Zeitschrift f. Psychologie*, VI, 340, 417). According to him, the single sensations of the several notes lie unaltered in the chord-sensations; but his analysis of the phenomenon is vitiated by his non-recognition of the fact that the *same objects* (*i. e.*, the notes) *can be known* representatively through one compound state of mind, and directly in several simple ones, without the simple and the compound states having strictly anything in common with each other. In Meinong's earlier work, *Ueber Begriff und Eigenschaften der Empfindung* (*Vierteljahrschrift*, vol. XII), he seems to me to have hit the truth much better, when he says that the aspect *color, e. g.*, in a concrete sensation of *red*, is not an abstractable part of the sensation, but an *external relation of resemblance* between that sensation and other sensations to the whole lot of which we give the name of colors. Such, I should say, are the aspects of *c, e, g* and *c'* in the chord. We may call them *parts* of the chord if we like, but they are not *bits* of it, identical with *c's, e's, g's* and *c's* elsewhere. They simply resemble the *c's, e's, g's* and *c's* elsewhere, and know these contents or objects representatively.

pressing them, make them self-contradictory (as the associationist psychology does when it calls them many ideas fused into one idea) or pretend to explain them (as the soul-theory so often does) by a barren verbal principle.

My intention was a good one, and a natural science infinitely more complete than the psychologies we now possess could be written without abandoning its terms. Like all authors, I have, therefore, been surprised that this child of my genius should not be more admired by others—should, in fact, have been generally either misunderstood or despised. But do not fear that on this occasion I am either going to defend or to re-explain the bantling. I am going to make things more harmonious by simply *giving it up*. I have become convinced since publishing that book that no conventional restrictions *can* keep metaphysical and so-called epistemological inquiries out of the psychology books. I see, moreover, better now than then that my proposal to designate mental states merely by their cognitive function leads to a somewhat strained way of talking of dreams and reveries, and to quite an unnatural way of talking of some emotional states. I am willing, consequently, henceforward that mental contents should be called complex, just as their objects are, and this even in psychology. Not because their parts are separable, as the parts of objects are; not because they have an eternal or quasi-eternal individual existence, like the parts of objects; for the various 'contents' of which they are parts are integers, existentially, and their parts only live as long as *they* live. Still, *in* them, we can call parts, parts.—But when, without circumlocution or disguise, I thus come over to your views, I insist that those of you who applaud me (if any such there be) should recognize the obligations which the new agreement imposes on yourselves. Not till you have dropped the old phrases, so absurd or so empty, of ideas 'self-compounding' or 'united by a spiritual principle;' not till you have in your turn succeeded in some such long inquiry into conditions as the one I have just failed in; not till you have laid bare more of the nature of that altogether unique kind of complexity in unity which mental states involve; not till then, I say, will psychology reach any real benefit from the conciliatory spirit of which I have done what I can to set an example.

## CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF COLUMBIA COLLEGE. (III.)

### EXPERIMENTS ON DERMAL SENSATIONS.<sup>1</sup>

BY HAROLD GRIFFING.

#### *The Relation between the Intensity of the Stimulus and its Estimated Intensity.*

Two stimuli differing greatly in intensity were successively applied to the hand of the observer, and he was required to judge how much greater one was than the other. The pressure was given by weights placed in the pan of a balance, and was transmitted to the hand by a wooden rod attached to the pan. The stimuli were 2, 10, 50, 250, 1250, and 1800 grams. The area of stimulation was that of a circle 4 mm. in diameter. The experiments made on four observers showed that on the average 10 g. was considered about twice as heavy as 2 g.; 50 g. twice as heavy as 10 g.; 250 g. three times as heavy as 50 g.; 1250 g. five times as heavy as 250 g.; and 1800 g. three times as heavy as 1250 g. It thus appears that for low and moderate intensities the estimate of intensity increases much more slowly than the objective intensity; but as the stimulus approaches the pain threshold, the reverse appears to be the case. Individuals differ, however, in their underestimation of low intensities, and also, but to a greater degree, in their overestimation of high intensities.

#### *The Discrimination of Weights of Different Intensities.*

Cylindrical boxes filled with shot served as stimuli. The method used was that of right and wrong cases; that is, the stimuli were placed successively upon the hand, and the ob-

<sup>1</sup> A full account and discussion of these experiments will be found in the writer's dissertation, *On Sensations from Pressure and Impact*. Supplement Monograph (No. 1) to this REVIEW.

server was asked to decide which was heavier. The accuracy of discrimination is measured by the probable error, or that increment which the observer perceives correctly 75% of the time.<sup>1</sup> Thus the greater the probable error the less the accuracy of discrimination. The stimuli varied from 100 to 3200 g., no more than four intensities being used for any one observer. The results of 9040 experiments made on 5 observers showed that the probable error for pressure stimuli tends to increase in proportion to the intensity of the stimulus within the approximate limits 300-3000 g. For low intensities the probable error increases much more slowly than the stimulus. For 5-7 g. the probable error for a good observer was  $\frac{1}{2}$  of the stimulus. For high intensities also there seems to be a similar tendency, but it is not so marked. As illustrative of our results, we give the probable errors in grams for McW.: for 100 g., 19; for 500 g., 36; for 1500 g., 112; for 3200 g., 193. The average value of the probable error for all stimuli (100 g. and above) and all observers was approximately  $\frac{1}{3}$  of the stimulus. That is we can, on the average, judge correctly whether one stimulus is heavier or lighter than another 75% of the time when the stimuli are in the ratio 9:10.

In these experiments the constant error, or tendency to overestimate the second stimulus, was found to be for some persons very great, running as high as  $\frac{1}{2}$  of the stimulus. The constant error is more variable than the probable error; the expression 'constant error' is thus quite misleading. The constant error seems to be greater for observers having a large probable error. A great constant error for pressure is not necessarily accompanied by a similar overestimation for lifted weights.

The degree of confidence was studied by having the observers say *a*, *b*, *c* and *d*, according as they were certain, quite confident, less confident, or doubtful. Individuals differ greatly in their confidence, the percentage of wrong judgments of which observers were confident varying from 2% to 33%. The probability of correctness when confident

<sup>1</sup> This quantity has been considered to be equivalent to the least noticeable difference. It is doubtful, however, if such a relation can be justified.

was for most observers about .8 to .9. There appears to be no relation between these quantities and the accuracy of discrimination. The percentage of correct guesses varied from 52% to 70%, the average being 59%.

*The Place of Stimulation.*

The accuracy of discrimination for weights of 100 g. or more is not for two observers appreciably different for the palm of the hand, the back of the hand, and the volar surface of the third phalanx of the index finger. For 5-7 g. it was found at first to be much less for the back of the hand and wrist than for the index finger of one observer, but to increase greatly by practice. Stimuli of low intensity, 5 and 100 g., when placed on the forearm, tended to be judged lighter than when placed on the finger. This result was obtained by placing a weight first on the finger and then on the arm, increments being added until the weights seemed equal.

The writer tested the sensitiveness to pain at different parts of the body by the algometer.<sup>1</sup> It was found that the sensitiveness is greatest where the skin is thin and not separated from the bone by other tissues. Among the most sensitive parts are the upper regions of the head, whereas the palm of the hand, the thigh and the heel are among the least sensitive parts.

*Sensations from Impact.*

The tactile threshold for pressure stimuli without movement was found by observing the angular elevation of a bristle which was attached at one end to a wooden handle, and at the other could transmit pressure to the skin. In this way it was found that .4 g. is about as easily perceived when movement is thus excluded, as is .01 g., when the stimulus is placed carefully upon the hand. The difference in the results is due to the sensory effect of movement.

By dropping weights upon the hand, the heights were found at which different weights caused pain. The weights were 25, 100, 200 and 300 g. The area of stimulation was

<sup>1</sup> An instrument by which pressure could be exerted up to 15 k.

constant, a circle about 1 cm. in diameter. The results of 60 measurements showed that the product of the mass and height pain-thresholds is fairly constant. As the height through which a body falls is proportional to the square of the velocity, the pain threshold and therefore the intensity of pain, depend as much upon the square of the velocity as upon the mass of a striking object.

By the method of right and wrong cases we studied the accuracy of discrimination for impact stimuli. The results of 800 experiments showed that a weight of 50 g., falling through 17.5 cm., is judged about as well as 1000 g. without movement. The average probable error for pressure only was  $\frac{1}{11}$  of the stimulus for S. F., and  $\frac{1}{14}$  for L. F. For impact the corresponding values were  $\frac{1}{10}$  and  $\frac{1}{12}$ .

In 900 experiments, carried on in the same way, the weight was kept constant and the observer required to estimate differences in the intensity of the blow due to differences in height and therefore velocity. The results were compared with those based upon the same number of experiments on the same observers, in which the height was constant and the weight variable. We found that, on the whole, differences in weight are judged less accurately than differences in velocity, but more accurately than differences in the square of the velocity. But great individual variations occur.

Experiments were also made on the intensive effect of the weight as compared to that of the velocity. A 100 g. weight having fallen upon the hand from a height of 5 cm., the height was found at which 25 g. would cause a sensation of the same intensity. Here also observers differed greatly. The average height for 5 observers was 38 cm., the maximum being 58, the minimum 20 cm. Hence the mass has in general greater intensive effect than the height or the square of the velocity. Otherwise the average height found would be about 20 cm. On the other hand, the mass has less effect than the velocity or square root of the height.

#### *The Area of Stimulation.*

In the experiments on Weber's law two areas were used, 8 sq. cm. and .12 sq. cm. approximately. It was found that

on the whole this difference of area did not affect the accuracy of discrimination for weights. Individual variations, however, were very marked.

If stimuli of the same weight, but different areas, be placed successively upon the hand, the stimulus applied on the smaller area will be overestimated. By applying the method of right and wrong cases we measured this overestimation. The results of 400 experiments on one observer gave an overestimation of  $\frac{1}{3}$  of the stimulus at 200 g. Experiments by a different application of the method of right and wrong cases on 5 observers gave about the same result, except that one observer showed a tendency to underestimate, rather than overestimate, the stimulus applied to the smaller area. By a third method, however, we found a decided overestimation for only 2 out of 5 observers. From these experiments on 10 observers, we conclude that this tendency is by no means universal.

The effect of alterations in the intensity of pressure on the accuracy of discrimination of areas was investigated by the method of right and wrong cases, differences in area being judged instead of differences in intensity. The standard areas used were 1 and 8 sq. cm. and the intensities 200 and 800 grams. The results of 1900 experiments on 3 observers showed that the accuracy of discrimination for areas was, on the average, about  $\frac{1}{3}$  greater for 200 g. than for 800 g.

By placing thin circular cards upon the hand and applying pressure upon these, we studied the effect of variations in the area on the so-called tactile threshold. The areas were approximately 1 mm., 10 mm. and 90 mm. The averages of the corresponding threshold values, based upon 60 experiments, were for F., .2 g., .9 g. and 1.9 g.; and for the writer, .5 g., 1.4 g. and 1.6 g. Thus the smaller the area the greater the probability that stimuli of low intensity will be perceived.

In a similar manner the relation of the pain threshold to the area of stimulation was investigated. The average values of the pain threshold, based upon 80 experiments on two observers, were: for 10 mm., 1.4 kilog.; for 30 mm.,

2.8 kilog.; for 90 mm., 4.4 kilog.; and for 270 mm., 6.6 kilog. Thus the pain threshold increases with the area; but, like the tactile threshold, much more slowly than in direct proportion.

*The Time of Stimulation.*

The sensory effect of pressure stimuli of low intensity was found to depend upon the rate at which the pressure was increased. The instrument used was that referred to in the experiments already described on the tactile threshold. By this pressure was exerted upon the palm of the observer's hand up to .4 g., at different rates of increase. These rates were approximately .05 g., .3 g. and 2 g. per second. The corresponding percentages of times the stimulus was perceived in 300 experiments on 2 observers were 6%, 32%, and 82%. Thus the greater the rate of increase the greater the probability of perception.

The time in which dermal stimuli of different intensities cause pain was found in the following manner. Different weights were placed in a balance pan so as to press upon the palm of the hand, and the time was noted which elapsed before the appearance of pain. The pressure was communicated from the pan to the hand by a wooden rod fastened to the pan. The diameter of the base was 1.5 mm. The averages in seconds, based upon 80 experiments on 2 observers, are as follows: for 100 g., 230 sec.; for 200 g., 35 sec.; for 300 g., 10 sec.; for 500 g., 4.5 sec. It is evident, therefore, that the time as well as the area and intensity of stimulation determine the sensory effect. There is, however, an intensive limit, below which pressure stimuli never become painful. This is probably from 25 to 50 g. for the area used.

THE AFTER-IMAGE THRESHOLD.

BY SHEPHERD IVORY FRANZ.

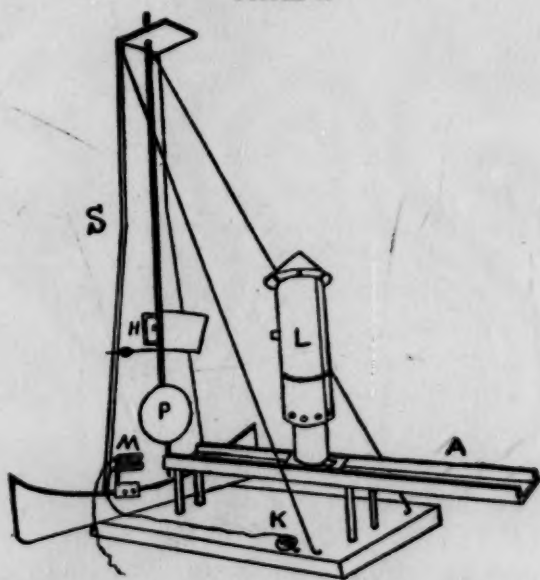
Ever since Aristotle described in his *De Somniis*<sup>1</sup> the appearance of an after-image, the phenomena have attracted

<sup>1</sup> This seems not to be generally known by German writers. Aubert and Helmholtz both credit Peiresc as being the first to mention after-images.

attention. St. Augustine mentions them, and in modern times such prominent men as Buffon, Goethe and Newton have described their appearance. But very little was accomplished beyond the making of theories until this century, when Plateau, Seguin, Fechner and others studied the color changes. Up to the present time practically nothing has been accomplished in the way of exact measurement.

The present paper gives the results of an attempt to measure the smallest amount of light which will produce an after-image. For this purpose three physical units had to be considered—the intensity of the light, its area, and the time of stimulation. The apparatus used was planned and formerly used by Prof. Cattell, but was adapted by the writer. It is represented in the accompanying cut.

FIGURE 1.



S is an upright iron screen pierced by a hole (H) through which the light from the hooded lamp (L) may pass to the observer on the other side of the screen. P is a seconds pendulum. To this is attached a piece of sheet iron which covers the hole when the pendulum is held up by the electromagnet (M). The key (K) which makes and breaks the current to the magnet (M) is managed by the experimenter, and

the pendulum is held up or let swing at his pleasure. By breaking and making the current the pendulum swings, permits the light to be seen by the observer for exactly one second, and is caught up again by the magnet. The lamp is moved along the arm (A), increasing or decreasing the intensity of the light. The opening (H) was covered with ground glass.  $\frac{1}{100}$  candle power was found a convenient intensity, this being increased by moving the lamp nearer the observer, and decreased by moving it away from the observer. The lamp was used at the distances  $\frac{1}{4}$ ,  $\frac{1}{2}$ , 1, 2 and 4 meters, and so far as the intensity decreases inversely as the square of the distance, the respective intensities would be  $\frac{4}{100}$ ,  $\frac{1}{100}$ ,  $\frac{1}{400}$ ,  $\frac{1}{1600}$  and  $\frac{1}{16000}$  candle power. The absorbing power of the ground glass was found to be 50%, whence the intensities were decreased by half—making the series— $\frac{2}{100}$ ,  $\frac{1}{100}$ ,  $\frac{1}{400}$ ,  $\frac{1}{1600}$ ,  $\frac{1}{16000}$  c. p. In the experiments on intensity, the time of exposure (one second) and the area (64 sq. mm.) were kept constant. For the experiments on area, the lamp was placed at a distance of  $\frac{1}{4}$  m., thus making the intensity  $\frac{4}{100}$  c. p., the time (one second) being the other constant. The area was changed by using different pieces of ground glass on which black paper blocked off all but the small area required. The areas used were 64, 16, 4, 1,  $\frac{1}{4}$ ,  $\frac{1}{16}$  sq. mm. When time was the changeable unit, the area (64 sq. mm.) and the intensity ( $\frac{4}{100}$  c. p.) were the constants. The series consisted of four times,  $\frac{1}{10000}$ ,  $\frac{1}{1000}$ ,  $\frac{1}{100}$  and 1 second. The shorter times were obtained by means of drop screens, made of pasteboard and weighted. As they did not fall in grooves there was no appreciable friction, and hence the real time practically corresponded with the theoretical time. The screen was on the side of the apparatus near the observer, and therefore is not shown in the cut. The time one second was given by the pendulum. As will be noticed, there was a common unit in the three series, *i. e.*, when the experiments were made with 1 sec., 64 sq. mm. and  $\frac{4}{100}$  c. p.

The experiments were conducted in a dark room, and all observations were made with the eyes open, so as not to disturb the after-image. A cloth curtain was hung across the room, shutting off from the observer everything but the

small opening in the screen. The observer's eyes were 30 cm. from the opening, his head being steadied by a support. Before any experiments were made a rest of ten minutes was taken to allow the observer's eyes to become accustomed to the darkness; between the disappearance of one after-image and the next stimulus there was a rest of thirty seconds. When the thirty seconds had elapsed a signal was given, five seconds were allowed for preparation, and the stimulus was produced.

Very few difficulties presented themselves, and of these the only one not overcome was the lack of a fixation point, as any fixation point was apt to produce a disturbing after-image. By practice, however, the observer learned to look in a certain way for the stimulus, and in the case of the writer not over five per cent. of the time were the eyes consciously focussed after any part of the light was seen. The kerosene lamp used was trimmed at the beginning of the experiments. By photometric determinations always made before a sitting and generally during and after the sitting, it was found that the light varied very little or not at all.

Four observers were tested, C., McW. and S. respectively with time, area and intensity. All were advanced students in psychology, and S. had had previous experience with after-images. F., the writer, was the fourth observer, the three series being made upon him.

The results of nearly 3,000 experiments are given in the following tables. In the first line the percentage of times an after-image was seen is given, and in the second line the average variation of the sets of ten trials; 100 experiments of each sort were made, excepting in those cases in which a different number is given in parenthesis.

Some preliminary experiments on area made on the writer bear out in general the results in the corresponding series. These experiments were made with an intensity of  $\frac{1}{10}$  c. p., so that they could not be combined with the others. The other constant was an exposure of one second. The same areas were used except that the  $\frac{1}{16}$  sq. mm. was omitted. Seventy experiments were made on each area. The results, with the average variations, are shown in the accompanying table.

## INTENSITY.

Intensity in candle power.	$\frac{2}{35}$	$\frac{1}{50}$	$\frac{1}{100}$	$\frac{1}{800}$	$\frac{1}{3200}$
S. { Percentage, { Variation,	100 — (80)	94 7	48 25 (110)	17 20.8 (110)	2 3.6
F. { Per cent., { Var.,	100 —	96 5.4	44 19.5 (130)	15.5 13.5 (130)	1 1.8

## AREA.

Area in square mm.	64	16	4	1	$\frac{1}{4}$	$\frac{1}{16}$	
McW. {	Per cent.,	100	90	72	52	27	20
	Var.,	— (50)	7.5 (80)	14.8	12.4	9.2	6
F. {	Per cent.,	100	96	88	57	31	8
	Var.,	—	4.8	8.8	13	15.4	8

## TIME.

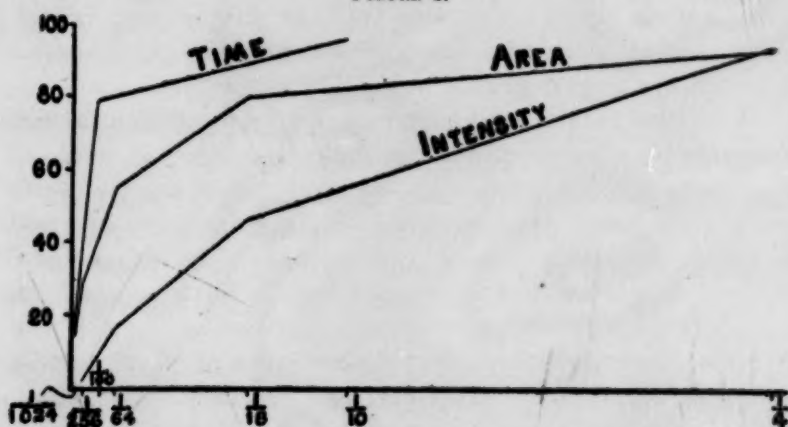
Time in seconds.	1	$\frac{1}{10}$	$\frac{1}{100}$	$\frac{1}{1000}$	
C. {	Per cent.,	97	95	75	12
	Var.,	3.5 (70)	7	17	10.8
F. {	Per cent.,	100	97	82.5	19
	Var.,	—	4.2	8.3 (120)	7.4

## AREA.

Area in sq. mm.		64	16	4	1	$\frac{1}{4}$
F.	Per cent.,	96	89	67	41	19
	Var.,	5	10	14.7	10	9.9

The results of the first three tables are represented graphically by the accompanying curves.

FIGURE 2.



The abscissa denotes respectively divisions of time, area and intensity, the ordinate the percentage of times an after-image appeared. The curves are not carried out to represent the greatest intensity, the greatest area and the greatest time. Each curve is the average of the two observers in that series, the close agreement of the observers making this method permissible. The figures on the abscissa represent the proportion of that stimulus to the greatest stimulus, taking respectively time, area and intensity as the variables.

If we regard the threshold as that intensity, time or area, which produces an after-image 75% of the number of stimuli, we conclude

- (1). That with an exposure of one second and an intensity of  $\frac{2}{15}$  c. p., the threshold is 4 sq. mm.
- (2). That with the area 64 sq. mm. and the intensity  $\frac{2}{15}$  c. p., the threshold is  $\frac{1}{100}$  second.

- (3). That with the area 64 sq. mm. and the time of exposure one second, the threshold is  $\frac{1}{100}$  candle power (approximately), or between  $\frac{1}{80}$  and  $\frac{1}{100}$  c. p.

If we substitute in our definition 25%, or 50%, or 90%, for the 75%, we but change the figures to suit the case.

It is worth noting that of the 1,500 cases when after-images were seen, but five were negative, a proof of the theory that the negative after-image is due to exhaustion of the eyes, the low intensities, the small areas and the short times not being sufficient to tire or exhaust the eyes. These five negative images were all seen toward the close of a sitting, when the eyes had been used for forty or fifty experiments, and all were with the greatest intensity, the longest time and the largest area.

With the results obtained we are able to make a further comparison—a correlation of our physical units in terms of the production of after-images—a purely psychological problem. How much time equals how much intensity or area? A glance at the curves and percentages shows that equal increments in area, intensity and time do not give equal results. If we represent our constants by the letters  $c$ ,  $c'$  and  $c''$  respectively for intensity, time and area, and let  $i$ ,  $t$  and  $a$  represent respectively  $\frac{1}{80}$  c. p.,  $\frac{1}{100}$  sec. and  $\frac{1}{8}$  sq. mm., from the table of percentages we get the following approximate equations.—

$$i c = t c' = a c''$$

$$(2 i c) = (1.7 t c') = (4 a c'')$$

$$4 i c = 3.2 t c' = 16 a c''$$

$$8 i c = 10 t c' = 64 a c''$$

$$16 i c = 100 t c' = 256 a c''$$

The  $8 i c$  and the  $3.2 t c'$  represent  $\frac{1}{10}$  c. p. and  $\frac{1}{100}$  sec. (approximately). These figures and the second equation in brackets are supplied from the curves. The relations, then, may be stated as follows: "Squaring the time equals doubling the intensity or quadrupling the area," and vice versa, "reducing the area to one-fourth equals halving the intensity and taking the square root of the time." Whether this be a chance relation or a general one throughout the phenomena of after-images cannot be dogmatically stated now. The writer has in view the further study of this problem.

## THE NORMAL DEFECT OF VISION IN THE FOVEA.

BY CHRISTINE LADD FRANKLIN.

When the fact that the retina contains a substance which is chemically acted upon by light was first announced, it seemed that the secret of the transformation of energy of wave-motion into something capable of being transmitted along the nerve fibres and affecting the conscious organism as the sensation of light had been definitely, at least in its rough stages, unravelled. But immediately difficulties appeared: the substance could not be detected in the cones, and it was therefore apparently wanting in the fovea, the spot of most acute vision; and, moreover, certain classes of animals had retinas which contained none of the substance. It was therefore certain that the visual purple was not essential to vision, and the intense interest which it had at first aroused fell wholly into abeyance.

Prof. Ebbinghaus has recently returned to the subject, and has proposed to account for the apparent colorlessness of the cones by assuming in them a second substance of such a color as always to mark the presence of the visual purple. The visual purple (or visual blue, as it must be considered for this purpose, although its real color is only a very slightly bluish-red) and its product, the visual yellow, are the source of the sensations of yellow and blue respectively; the imaginary substance is, in its two stages, the source of the sensations red and green, and is for that purpose first green and then red in color. Now, a green and a purple substance, when present together, might, it is true, produce a colorless mixture, since purple and green are complementary colors; but a moment later these two substances have become respectively yellow and red. What

becomes of the complementariness then?—or when one is green and the other yellow?—or when one is red and the other purple? Or must we suppose that, although thousands of eyes have been examined, first and last, after every possible degree of exposure to light, and to color, still chance has brought it about that no stage of this series of processes has ever been lighted upon except the first? So short-sighted a theory as this,—one in which we must so carefully refrain from going beyond the first step of the imagined process,—has probably never before been seriously proposed for acceptance.

But the suggestion of Prof. Ebbinghaus has had this good effect, that it has induced Prof. König to undertake an accurate determination of the relative absorption of the visual purple for different kinds of homogeneous light.<sup>1</sup> He proposed the question as a subject of investigation to Dr. Abelsdorff and Frl. Köttgen. A spectro-photometer especially designed for the purpose was constructed, and it was hoped that the skill and experience gained in the study of the visual purple of the frog they might, in course of time, be able to apply to a human retina, if good luck should throw one in their way. But, as it happened, the apparatus was no sooner set up in one of the dark rooms of the laboratory than they received word that a human retina was to be at their disposal; and Dr. Abelsdorff being suddenly called away, the study of it was carried out by Prof. König and Frl. Köttgen. The patient to whom the eye belonged remained in absolute darkness for twenty hours before the operation. The eye was extracted by the light of a sodium flame, put at once into an intensely black box, and rapidly conveyed to Prof. König's laboratory. Here it was opened, twenty minutes after leaving the living body, with all the necessary precautions, by an oculist who had already made himself familiar, by means of the ophthalmoscope, with the exact position of the melano-sarcoma which had caused the eye to be extracted. The entire retina, with the exception

<sup>1</sup> *Ueber den menschlichen Sehpurpur und seine Bedeutung für das Sehen.* Nach gemeinschaftlich mit Frl. Else Köttgen ausgeführten Versuchen. Sitzungsber. d. Akad. d. Wissensch. zu Berlin, 21 Juni, 1894.

of the diseased portion, was put into a solution of gallic acid, and after filtration a sufficient amount of the extract was obtained to fill twice the minute absorption-box of the spectrophotometer. With the first filling the absorption of the visual purple was obtained and compared with the absorption of the (not absolutely clear) solution which remained after the purple (crimson) color had been wholly bleached out; the second filling sufficed for a redetermination of the absorption of the visual purple, and for that of the visual yellow, which was obtained after the purple had been bleached for that color. (The two determinations of the absorption of the purple substance are in close agreement with each other.)

It was at once evident that the absorption distribution in the spectrum of the purple substance coincided roughly with the spectral distribution of brightness for the congenitally totally color-blind, and also with the spectral distribution of brightness for the normal eye (as well as for the partially color-blind) at a very faint degree of luminosity. The suggestion was a natural one that it is the vision of the totally color-blind, and of the normal eye in a faint light, which is dependent upon the absorption of light by the visual purple. The curves of sensation in these two cases were reduced to a spectrum of equal distribution of energy by means of Prof. Langley's determination of the distribution of energy throughout the spectrum. Correction was also made for the absorption of the macula lutea and for that of the crystalline lens (freshly determined for an individual of the proper age). It then became evident that the coincidence between the three curves is remarkably close. (That the two curves of sensation referred to are in close agreement with each other had, of course, already been shown by Hering.) It was evident, that is, that the absorption in the purple substance is very exactly proportional to the value of light as an exciter of sensation (1) in the totally color-blind, and (2) in all other eyes at an intensity so faint that colors are no longer visible. Prof. König had also convinced himself of the existence of a similar coincidence between the absorption of the visual yellow and the blue constituent of

the colors of the spectrum as already determined by himself and Dieterici (*Zeitsch. f. Psych. u. Phys. der Sinnesorgane*, IV., S. 241).

But the difficulty still remained which had originally caused the visual purple to fall into neglect,—the substance is apparently wanting in the cones, and therefore in the fovea. To meet this difficulty two assumptions were possible: either, that the cones do contain the purple substance, but in so decomposable a form that it can never be detected objectively, no matter what the precaution used in extracting the eye; or, that the eye is actually blind in the fovea in the two cases in question. In favor of the first assumption was the fact that, if the yellow substance is really the source of the sensation of blue, then it must be supposed to exist in a *less* decomposable state in the periphery of the eye to account for the fact that we are then nearly blind to blue;<sup>1</sup> it therefore 'lies near' to assume (when some assumption is absolutely necessary) that it exists in a *much more* decomposable state in the fovea, and that it has for this reason hitherto escaped detection. But I was most anxious to put the second of these assumptions to the test,—the more so as I had already made the prediction that the cause of total color-blindness is a defective development of the cones;<sup>2</sup> and also that the function of the visual purple is to render possible that form of vision which does not exist until after a delay of twenty minutes or so in a dark room;<sup>3</sup> both predictions being naturally suggested by my theory of light-sensation. I had also pointed out, in the last-mentioned paper, that the visual purple cannot exist in the cones, even in a bleached-out state, because the visual purple is fluorescent, and the more so the more it is bleached out, while the

<sup>1</sup> Gad, in his criticism of the papers of König and Zumft, about to be mentioned, implies (p. 499) that Prof. König found the blue-blindness of the fovea forced upon him by his hypothesis regarding the function of the visual purple and of the visual yellow. That was not the case; Prof. König had adopted the first of the two assumptions here affirmed to be possible, and it was only some six weeks later that the defective vision of the fovea was discovered.

<sup>2</sup> *Zeitsch. f. Psych. u. Phys. der Sinnesorgane*. Bd. IV. s. 9.

<sup>3</sup> *Professor Ebbinghaus' Theory of Colour Vision*. MIND, N. S. Vol. III, p.

fovea remains as a dark spot in the ultra-violet rays of the spectrum; and the more strikingly dark the more the rods in the neighborhood have become fluorescent (p. 100). On the other hand, Prof. König pointed out to me that even if vision should be wholly wanting in the fovea of a totally color-blind individual, it would hardly be possible to detect it, for he would unquestionably have acquired the habit by avoiding the use of this spot. This suggestion was, therefore, not immediately carried out. But it was arranged that I should take for the subject of my investigation for the summer a re-determination of the threshold of sensation for different parts of the retina and for different kinds of monochromatic light. A plan of work was built up in two of the dark rooms of the laboratory, and I have to express my gratitude to Prof. König for his untiring patience in assisting me to overcome the difficulties which one after another presented themselves.<sup>1</sup> The preliminary observations for eliminating the sources of error consumed some time, and I then made a first determination of the variation in the intensity of light necessary in order to be just perceptible, or of its inversion—the sensitiveness of the eye to faint impressions—at different distances from the fovea. I even drew the curves, and found them to present a maximum at a distance of about  $25^{\circ}$ , at which point the sensitiveness of the eye is about four times as great as at the fovea, while at a distance of  $50^{\circ}$  the sensitiveness is still about twice as great as at the fovea. E. Fick found the maximum to be at about  $15^{\circ}$ , but that was without making correction for the diminished area of the pupil of the eye when light enters it very much from the side. The shape of the curves is not noticeably different for different parts of the spectrum. These curves are a representation of the diminished sensitiveness in the region of the fovea, which has long been known, and which has been especially forced upon the attention of astronomers when looking for faint stars with the naked eye. I had been in the end for several weeks at work in my dark room for the express purpose of finding that the fovea is *blind* to impressions so faint as those with which I was occupied, before

<sup>1</sup> The full results of this investigation will be published later.

I found it; although, after it has once been seen, it seems incredible that it can ever have been overlooked. It finally dawned upon me—not that the bright point directly looked at was invisible—but that by giving what I can only describe as a certain curious twist to the eye, a certain bright point could be caused to disappear.<sup>1</sup>

The reason that the 'normal night-blindness of the fovea,' as this insensitiveness to the faint-light sensation may best be called, has been completely overlooked by all other observers, and also by E. Fick and by Kirschmann, who have made a special investigation of the threshold of sensation for different parts of the retina, is very plain: the unconscious ego, which takes so large a part in regulating the action of even the voluntary muscles, is well aware of this blindness, and takes pains that an image of a small object shall almost never fall upon this spot. In a faint light, *to look at*, which is usually a phrase of two-fold significance, meaning, namely, to turn the eye in such a way that its power of seeing is a maximum; and also to turn the eye so that the image of the object looked at falls on the fovea, has now the two elements of its significance disjoined; when vision is at a maximum (or when it is possible at all), it is necessary that the image should fall a little to one side of the fovea, and that is the motion with which the subjective feeling of fixation is associated. Not only did the faint object which I was engaged in observing disappear, but also the two (much brighter) spots of phosphorescent paste (which are used in order to secure a fixation-point halfway between them) could be made to completely vanish by 'looking at' them, in the new sense of that phrase. This phosphorescent matter gives a spectrum which is almost wholly blue.

Having convinced myself of the existence of this faint-light foveal blindness, it was necessary to devise a method by which the total blindness of the fovea of the totally color-

<sup>1</sup> This motion of the eye can be facilitated if one brings in the aid of a strong desire not to see the point. This would seem to show that the knowledge of the existence of this blind spot, while almost wholly below the level of consciousness, is yet not altogether withdrawn from an interaction with the conscious content of the organism.

blind patient, who was soon to return to Prof. König's laboratory, could be demonstrated. It was not permitted to subject his eyes to any strain, and it was not probable that a rather feeble boy of thirteen could easily learn to execute a motion which had hitherto been absolutely avoided, not only by him but by all the rest of the world; and which, besides, there was no possibility of describing to him. But it naturally suggested itself to me very soon that it would only be necessary to give him a group of closely contiguous isolated bright points to look at, and that chance would see to it that one or the other of them should now and then fall into the dark hole of his fovea. The same device has proved effective for exhibiting the faint-light blindness to a person who has not yet learned to execute the motion of the eye necessary to cause a single spot to disappear. Prof. König at once made use of this method to show that even the most intense blue that could be thrown into the field of his spectro-photometer, by the light of the oxyhydrogen blow-pipe, is insufficient to cause any sensation whatever in the fovea. No difficulty was experienced in demonstrating the total blindness of the totally color-blind boy in this spot, although it was quite impossible to get him to experience the invisibility of a single bright point when only one was in the field. This individual had a definite spot at one side of the fovea, which he constantly made use of as a fixation-spot; the nystagmus, which is a common accompaniment of total color-blindness, is readily explained as the expression of there being no such favored substitution fovea. The remarkable diminution of visual acuity on the part of such patients, which has not hitherto been understood, is seen to be very natural when it is known that their fovea is not in a condition to perform its function. Prof. König proceeded at once to make a series of color-equations in the fovea—a work of extreme difficulty—from which it appears that the condition, which extends over an area of from 55' to 70', is that of a typical blue-blindness.

To the facts already described, Prof. König adds a contribution recently made by himself and Dr. Zumft,<sup>1</sup> by which

<sup>1</sup> *Ueber die lichtempfindliche Schicht in der Netzhaut des Menschlichen Auges.* Sitzungsberichte d. Akad. d. Wissensch. zu Berlin, 24 Mai, 1894.

they would seem to have shown that light of different colors is perceived in different layers of the retina, and blue distinctly in front of green, yellow and red. The method consists in throwing two shadows of a blood-vessel upon the back of the retina, by means of two holes in a card, which is constantly moved to and fro in the front focal plane of the eye. The distance apart of the two shadows they were able to measure, and they found it to be different for differently colored homogeneous light; and the calculated distance of the blood-vessel from the layer of retina which is affected by the light, they found to be, for several portions of the spectrum examined:

$\lambda$ 670 . . . . .	0.44 mm.
590 . . . . .	0.44
535 . . . . .	0.41
486 . . . . .	0.38
434 . . . . .	0.36
White, . . . . .	0.41

Prof. König interprets this to mean that the space between the layer in which blue is perceived, and that in which red is perceived, is *greater* than the thickness of the end members of the rods and cones, and hence that one must infer that the pigment epithelium also is a layer sensitive to light. It would seem, however, that there must be something about these experiments the meaning of which is not yet wholly cleared up, for the length of the outer member of a rod is only .025 to .03 mm., and that of an epithelium cell is only about half as much again. They do not, therefore, together form a layer of sufficient thickness to take in the difference of .08 mm., which the observations require. The experiment, therefore, proves too much. Again, Prof. König's interpretation of the facts here enumerated, as meaning that the visual yellow is the source of the sensation of blue; that green, yellow and red are all perceived in the pigment-epithelium, and that the *cones* are merely lenses for concentrating light upon the epithelium cells, makes no provision for the nerve-conduction of any effect of light in the epithelium. In the fovea there would be absolutely no means of such conduction except by way of the cones, and

it is difficult to conceive that organs which are performing the part of lenses should also be able to function as conductors.<sup>1</sup> Again, the recent brilliant work of Ramon y Cayal and others on the minute anatomy of the retina discloses such close similarity (together with a perfectly definite difference) between the rods and the cones, as regards structure and connections, as to make it very unnatural to assign to them functions of a widely different nature. Prof. König says (p. 4) that the results here communicated "are in contradiction (1) with the theories of Hering and Ebbinghaus, according to which a single substance forms the basis of the red and green sensations on the one hand, and of the blue and yellow sensations on the other hand; and (2) with the theories of Donders, Wundt and Franklin, according to which all colors are perceived in a single substance." It is true that all these theories would be rather hard hit by these results, if the results themselves were not involved in some obscurity. As it is, however, it may perhaps be safe to wait until the discrepancies pointed out have been, to some extent at least, cleared up.

There is yet one more recent contribution from König's laboratory which has an important bearing upon the new facts already mentioned. Brodhun, and more recently Tonn, have shown that the Purkinje phenomenon consists in a change in the blue constituent of white light—the red and green remaining unchanged; this would seem to indicate that the increased amount of coloring matter in the rods, as the intensity of light begins to diminish, furnishes a means for an increased amount of absorption, and would seem to point, it must be confessed, to the rods as the seat, at least in part, of the sensation of blue.

Farther elements of the theory of light-sensation now advocated by Prof. König are these:

<sup>1</sup> Prof. Gad (whose paper has reached me since writing the above) makes the farther criticism that only the first surface of the pigment-cells would be available, because light cannot pass through even a very thin layer of the fuscine which gives them their dark color. But he apparently forgets that, under an ordinary degree of illumination, the pigment grains are nearly all heaped up *between* the visual elements, and that the body of the pigment cell is left almost free from them. (*Der Energieumsatz in der Retina*. Separat-Abzug aus Arch. f. Anat. u. Phys., 1894.)

1. The visual purple is the photo-chemical substance whose decomposition causes the faint light sensation. That sensation is in reality blue, although we are not aware of it.

2. The visual yellow is the source of the sensation of blue at ordinary intensities.

3. The white, and also all shades of grey, of an ordinary illumination, are of a very different origin from (a) the sensation of grey in a faint light, (b) the sensation of the totally color-blind, (c) the sensation of the normal eye in the periphery; they are (as in the original Young-Helmholtz theory) a synthesis in 'judgment' of the sensations red, green and blue.

As regards Prof. König's interpretation of the new facts, the following observations remain to be made:

(a). There is no occasion for assuming that the visual purple is, by its decomposition, the source of the sensation. All that is forced upon us is that absorption by the visual purple acts as a means of *re-inforcement* at a time when light would be too feeble to perform its function without the presence of a special agent for absorbing it. That the visual purple and the visual yellow should, by their decomposition, furnish the same sensation (blue) is very hard to believe, in view of the fact that the visual yellow is, beyond all question, itself one of the decomposition products of the visual purple, and that their decomposition products can therefore not possibly be the same.

(b). Becker's case of congenital monocular total color-blindness, many cases of acquired monocular total color-blindness, and the consciousness of every individual in a faint light, all speak against the hypothesis that blue, and not grey, is perceived under those circumstances; still more, the perfect conviction which one has that a bit of colored paper whose image is removed to the periphery of the eye fades into a grey which is indistinguishable from the grey of direct vision.

(c). There is no doubt whatever that the eye has a perfectly unimpaired vision for the whole length of the spectrum when the light is so strong that the rod-yellow has been completely bleached out. That can, therefore, not be the

photo-chemical substance for blue. The eyes of the totally color-blind undergo adaptation.<sup>1</sup> The rod-purple in their eyes, therefore, suffers changes in its quantity exactly as we should expect it to do from what we know of the substance elsewhere. There is, therefore, every reason to believe that it is, like all rod-purple which we have ever examined objectively, completely bleached out in a bright light, and hence that it is not the sensation-producing substance, but merely a means of re-inforcement for waning light.

(d). The fact that the adaptation-substance is purple in color serves a useful purpose. The most common faint light of nature is the faint light of dense forests, which is green. The rod-pigment is therefore especially adapted to the absorption of the only light which penetrates them. How completely the light at the bottom of forest trees has been sifted of the light which their leaves absorb has been shown quite recently by an investigation into the growth (or rather non-growth) of nearly all ground plants after the foliage has fully come out in the late spring.<sup>2</sup>

(e). Almost the only function of the extreme periphery of the eye is the *detection of motion*,—that is, the detection of changes in the distribution of light and shade. The changes in the rod-pigment bring about a constant complete adaptation to the *existing* pattern of light and shade,—build up a counter-pattern, so to speak, upon the surface of the retina,—and only a *new* distribution of light (*i. e.*, the entrance of an enemy upon the field) causes any sensation. This function of the periphery is facilitated by the fact, made out by Ramon y Cayal, that there are numerous large, horizontal connecting cells which must play the part of re-inforcing a sensation by spreading it over a wide area, at the same time that they diminish the sharpness of its localization; the indistinctness of vision in the periphery has

<sup>1</sup> Just before leaving Berlin in September I made a journey to the place where the color-blind boy above referred to was spending the summer, in order to determine this point. Hering mentions that his case could see better in a dark room than those having normal eyes, but he does not say whether his vision improved with time. (*Untersuchung eines total Farbenblinden*. Pfl. Arch. Bd. 54 S. 10.)

<sup>2</sup> Klebs: *Einfluss des Lichtes auf die Fortpflanzung der Gewächse*. Biol. Centralbl. XIII, 641.

long been known to be much greater than the indistinctness of the image formed there would account for.

(*f*). If the rod-pigment, in both of its stages, is merely a reinforcement agent, then all theories of light-sensation (except, indeed, that of Ebbinghaus, which loses whatever plausibleness it may be supposed to have had) may be considered to remain very much in the same condition in which they were before.

#### IV. PROCEEDINGS OF THE THIRD ANNUAL MEETING OF THE AMERICAN PSYCHO- LOGICAL ASSOCIATION.

PRINCETON UNIVERSITY, PRINCETON, N. J., 1894.

##### REPORT OF THE SECRETARY AND TREASURER FOR 1894:

The third annual meeting of the American Psychological Association was held at Princeton College, Princeton, N. J., on Dec. 27 and 28, 1894. Prof. William James, President of the Association, presided over the sessions, which lasted from 10.30 A. M. on Dec. 27 to 4.30 P. M. on Dec. 28. President Patton, of Princeton College, made an address of welcome on Thursday afternoon, and entertained the members of the Association in the evening, after the address of the President of the Association. Abstracts of the papers read at the meeting are subjoined. Papers by Prof. Starr and Prof. Hume were presented in the absence of their authors, and papers offered by Prof. Jastrow, Prof. Delabarre, Prof. Titchener, Mr. Pierce and Dr. Witmer were not read.

The members in attendance were: Alexander, Baldwin, Cattell, Chrysostom, Farrand, Hyslop, Franklin, James, Ladd, MacDonald, Marshall, Mead, Mezes, Mills, Miller, Newbold, Ormond, Pace, Royce, Sanford, Strong, Warren—twenty-two in all. In addition, the sessions were well attended by professors and advanced students from the different universities and colleges.

The following nominations for membership were made by the council, and the elections were made by the Association:

Prof. Archibald Alexander,	New York.
Dr. John Bigham,	University of Michigan.
Prof. Charles L. Dana,	Bellevue Medical College.

Mr. E. A. Kirkpatrick,	Winona, Minn.
Dr. A. Kirschmann,	University of Toronto.
Prof. S. E. Mezes,	University of Texas.
Mr. W. T. Shaw,	Wesleyan University.
Prof. James Seth,	Brown University.
Prof. Paul Shorey,	University of Chicago.
Prof. H. M. Stanley,	Lake Forest University.
Miss M. Washburn,	Wells College.

A constitution was adopted, as follows:

CONSTITUTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

ART. I. *Object*.—The object of the Association is the advancement of Psychology as a science. Those are eligible for membership who are engaged in this work.

ART. II. *The Council*.—A Council shall be elected from the members of the Association as an executive. The Council shall consist of six members, two being elected annually for a term of three years. The President shall be *ex-officio* a member of the Council. The Council shall nominate officers for the Association, shall nominate new members, and shall make other recommendations concerning the conduct of the Association. The resolutions of the Council shall be brought before the Association and decided by a majority vote.

ART. III. *Officers*.—There shall be annually nominated by the Council and elected by the Association a President, a Secretary, and a Treasurer, who shall perform the usual duties of these officers.

ART. IV. *Annual Subscription*.—The annual subscription shall be \$3, in advance. Non-payment of dues for two consecutive years shall be considered as equivalent to resignation from the Association.

ART. V. *Executive Committee*.—The President, the Secretary, and a member from the place where the meeting is held, shall be a committee to make necessary arrangements for the annual meeting.

ART. VI. *Proceedings*.—Such proceedings shall be printed by the Secretary as the Association may direct.

ART. VII. *Amendments*.—Amendments to the Constitution must be adopted by a majority vote at two consecutive annual meetings.

As prescribed by the Constitution, a Council was elected as follows:

Term expiring 1897:

Prof. G. T. Ladd, Yale University.

Prof. J. McKeen Cattell, Columbia College.

Term expiring 1896:

Prof. J. Mark Baldwin, Princeton College.

Prof. William James, Harvard University.

Term expiring 1895:

Prof. John Dewey, University of Chicago.

Prof. G. S. Fullerton, University of Pennsylvania.

Prof. J. McKeen Cattell was elected President and Prof. E. C. Sanford Secretary and Treasurer for the coming year.

An invitation was received from the American Society of Naturalists, inviting the Association to affiliate with it. The question was referred to the Council, with power to act. Invitations were received for the meeting of 1895 from Harvard University and the University of Chicago. The decision as to place of meeting was left with the Council, with the recommendation that the convention meet, if possible, at the same time and place as the Society of Naturalists. It was resolved that the minutes should be printed in such journals as were prepared to print them in full.

The report of the Treasurer is as follows:

Receipts:

Balance on hand, . . . . .	\$69.50
2 dues, 1893, . . . . .	6.00
38 dues, 1894, . . . . .	114.00
Sales of Proceedings, . . . . .	1.60
	<hr/>
	\$191.10

## Expenditures:

Printing Proceedings for 1893, as per	
Messrs. Macmillan & Co.'s voucher,	\$55.93
Postage, expressage and stationery, .	8.00
	<hr/>
	\$63.93
Balance on hand, . . . . .	\$127.17

The account was audited by the Council and approved.

J. McKEEN CATTELL,  
Secretary, 1894.

## ABSTRACTS OF PAPERS.

- (1.) *The Knowing of Things Together.* Address by the President, Prof. WILLIAM JAMES, Harvard University.

The synthetic unity of consciousness is one of the great dividing questions in the philosophy of mind. We know things singly through as many distinct mental states. But on another occasion we may know the same things together through one state. The problem is as to the relation of the previous many states to the later one state. It will not do to make the mere statement of this problem incidentally involve a particular solution, as we should if we formulated the fact to be explained as *the combination of many states of mind into one*. The fact presents itself, in the first instance, as *the knowing of many things together*, and it is in those terms that the solution must be approached.

In the first place, *what is knowing?* 1. *Conceptual* knowing is an external relation between a state of mind and remote objects. If the state of mind, through a context of associates which the world supplies, leads to the objects smoothly and terminates there, we say it knows them. 2. *Intuitive* knowing is the identity of what, taken in one world-context, we call mental content and in another object. In neither 1 nor 2 is there involved any mysterious self-transcendency or presence in absence. 3. This mystery does, however, seem involved in *the relation between the parts of a mental content itself*. In the minimum real state of consciousness, that of the *passing moment*, past and present are known

at once. In desire, memory, etc., earlier and later elements are directly felt to *call for* or *fulfil* each other, and without this sense of mutuality in their parts, such states do not exist. Here is presence in absence; here knowing together; here the original prototype of what we *mean* by knowledge. This ultimate synthetic nature of the smallest real phenomenon of consciousness can neither be explained nor circumvented.

We can only trace the particular conditions by which particular contents come thus to figure with all their parts at once in consciousness. Several attempts were then briefly passed in review. Mere synchronical sense-impression is not a sufficient condition. An additional inner *event* is required. The event has been described: *physiologically* as 1) 'attention;' as 2) ideational processes added to the sensorial processes, the latter giving unity, the former manyness; as 3) motor synergy of processes; *psychologically* as 4) the thinking of relations between the parts of the content-object; as 5) the relating of each part to the self; *spiritually* as 6) an act of the soul; *transcendentally* as 7) the diminution (by unknown causes, possibly physiological) of the obstruction or limitation which the organism imposes on the natural knowing-of-all-things-together by an Absolute Mind. For transcendentalism the problem is, 'How are things known separately at all?'

The speaker dealt with these opinions critically, not espousing either one himself. He concluded by abandoning the attempt made in his *Principles of Psychology* to formulate mental states as integers, and to refer all plurality to the objects known by them. Practically, the metaphysical view cannot be excluded from psychology-books. 'Contents' have parts, because in intuitive knowledge contents and objects are identical; and Psychology, even as a 'natural science,' will find it easier to solve her problem of tracing the conditions that determine what objects shall be known together, by speaking of 'contents' as complex unities.

[The address is printed in full in the *Psychological Review* for March, 1895.]

- (2.) *Minor Studies, and Notes on New Apparatus.* By Dr. E. C. SANFORD, Clark University.

The four papers reported were on the following topics: (1) Comparative Observations on the Indirect Color Range of Children, Adults, and Adults Trained in Color, by Geo. W. A. Luckey. (This study was made in the Psychological Laboratory of the Leland Stanford, Jr., University); (2) A Study of Individual Psychology, by Miss Caroline Miles; (3) The Memory-span and Attention, by Dr. Arthur H. Daniels; (4) On the Least Observable Interval between Stimuli addressed to Disparate Senses and to Different Organs of the same Sense, by Miss Alice J. Hamlin; (5) Notes on the Binocular Stroboscope, a Model of the Hemispherical Field of Regard, and Diagrams for an Optical Illusion, by E. C. Sanford. [All of these papers are published in full in the *American Journal of Psychology*, Vol. VI., No. 4, Jan., 1895.]

- (3.) *The Psychic Development of Young Animals and its Physiological Correlation.* By T. WESLEY MILLS, Professor of Physiology in McGill University, Montreal.

As the comparative method, embryology and the doctrine of organic evolution have revolutionized biology, it must be expected that they or their analogies will at least greatly modify modern psychology. To learn how and when psychic processes originate is a long step towards understanding them; and as these processes in animals lower in the scale than man are presumably simple, it is desirable that they be studied both in the mature animal and in the young developing one. Accordingly the writer has for some years been engaged in this task, and has now made fairly complete researches on the psychic development of the dog, cat, rabbit, guinea-pig, etc.

An attempt has been made to keep a record in the form of a diary, not only of psychic, but of contemporaneous physical changes. A special series of experiments has been made on the brains of young animals, with a view of determining when cortical localization is established, in what order, etc. This work is not yet complete. Incidentally,

the subject of localization in the mature animal has been investigated, and some generally accepted conclusions found unreliable, as well as others confirmed.

(4.) *On the Distribution of Exceptional Ability.* By Professor J. McKEEN CATTELL, Columbia College.

A study of the mental traits and of the works of great men forms an interesting chapter in psychology; and while we are undertaking to make psychology an exact science, it is an advantage to secure quantitative results. When anecdotes are published telling us that certain great men have inherited or bequeathed their talents, were insane, immoral, precocious, versatile or the like, it is of interest; but we sometimes imagine that other examples might be quoted with opposite results, or similar traits found in ordinary people.

We need to be able to affirm that a man, who has accomplished work making him eminent, is more likely to be insane (according to a proper definition of insanity) than the average man, in a given ratio; and that this ratio varies in such and such a way for men whose work or character was of a given definable sort. And so in all cases quantitative results should be secured. We should be able to say that a man who is a great painter is just so much more likely to be a great poet as well, than is a great soldier, or than is the average man.

The first requirement for such a study is a list of great men secured by an objective method. The 1000 most eminent men have been selected by collating the space given to them in different biographical dictionaries and encyclopædias. The method secures impartiality and an assignable degree of accuracy, it being possible to give a probable error to each man. The list, of course, only gives a man's place in contemporary interest, but this would agree closely with the average verdict of the best judges as to his importance in history. The exact composition of the list is not indeed a matter of much importance for the end in view, an objectively selected list of great men being what is wanted.

The list was shown at the meeting, curves were exhibited demonstrating the distribution in time and race of the 1000 men, and attention was called to some facts brought out by the curves.

- (5.) *Sensibility to Pain by Pressure in the Hands of Individuals of Different Classes, Sexes and Nationalities.* By Dr. ARTHUR MACDONALD, Bureau of Education, Washington.

*Tabular Statement of Results.*

No.		Total No.	RIGHT HAND.			LEFT HAND.		
			No. requiring more pressure in r. h.	Totals in kilos.	Averages, kilos.	No. requiring more pressure in l. h.	Totals, kilos.	Averages, kilos.
1	2	3	4	5	6	7	8	9
1	American professional men, . .	20	14	74.50	3.72	5	65.25	3.26
2	American business men, . . . . .	14	6	85.25	6.08	6	87.75	6.05
3	American women, non-labor'g class	27	13	93.25	3.45	6	91.83	3.38
4	English professional men, . . . . .	17	9	88.50	5.20	6	87.25	5.13
5	English women, non-labor'g class	7	4	43.00	6.14	2	44.25	6.32
6	German professional men, . . . . .	6	5	31.25	5.20	1	29.00	4.83
7	Salvation Army members, London, . . . . .	8	6	73.25	9.15	2	51.00	7.62
8	Slum men in Chapel Rouge, Paris.	9	3	122.50	13.61	2	119.50	13.27
9	Boston Army of the unemployed, . .	34	16	332.50	9.77	14	333.75	9.81
10	Women in "Maisons de Tol- ance," Paris, . .	9	3	82.00	9.00	5	84.25	9.36
11	Epileptic patients, laboring people.	3	1	28.00	9.33	1	27.00	9.00
12	Odd ones, men, in Paris, . . . . .	7	4	28.25	4.03	3	26.25	3.75
13	Odd ones, men, in different countries, . . . . .	18	10	96.25	5.34	5	89.50	4.97
14	Men in general, .	142	76	1012.75	7.13	49	979.50	6.89
15	Women in general	46	21	230.50	5.01	15	233.08	5.06

The experiments reported were made incidentally upon different classes of people. Quite a number of university specialists interested in the subject were experimented upon.

The middle of the palmar fossa was chosen, and Professor Cattell's Algometer was employed.

Should these results prove to be generally true by experiments on larger numbers of people, the following statements would be probable: The majority of people are more sensitive to pain in their left hand (only exception is No. 10, cols. 4 and 7).

Women are more sensitive to pain than men (Nos. 14 and 15, cols. 6 and 9). Exceptions are: comp. Nos. 4 and 5, cols. 6 and 9. It does not necessarily follow that women cannot endure more pain than men.

American professional men are more sensitive to pain than American business men (comp. Nos. 1 and 2, cols. 6 and 9); and also than English or German professional men (comp. Nos. 1, 4 and 6, cols. 6 and 9).

The laboring classes are much less sensitive to pain than the non-laboring classes (comp. Nos. 1, 2 and 9, cols. 6 and 9).

The women of the lower classes are much less sensitive to pain than those of the better classes (comp. Nos. 3, 5 and 10, cols. 6 and 9). In general, the more developed the nervous system, the more sensitive it is to pain.

*Remark.*—While the thickness of tissue on the hand has some influence, it has by no means so much as one might suppose, *a priori*; for many with thin hands require much pressure (Nos. 5 and 10, cols. 6 and 9).

- (6). *The Freedom of the Will.* By BROTHER CHRYSOSTOM, Manhattan College, New York.

The positive results of the latest studies of the will, through introspection and experiment, are in striking accord with the teachings of the Schoolmen. The appetencies of Aristotle have been replaced by conation, which, if considered in the form of attention, is either *unequivocally conditioned*, and then corresponds to the sensitive appetite of scholastic philosophy, or is *equivocally conditioned*, and then does not essentially differ from the volition of earlier philosophers. But since equivocally conditioned attention may include among the objects attended to even the attending subject, it must be a spiritual action, for matter is incapable of such

reflexive process. In other words, the attending mind is a rational soul. In this light *apperception* may be characterized as the distinctive quality of conation. But apperception supposes at least such intellectual action as is contained in conception, and this in turn supposes sensation; and thus a point of contact is made with Münsterberg's theory.

Neither a purely autogenetic nor a purely heterogenetic theory of will accounts for all the facts. For conation is not a mere combination of sensations, nor a resultant of affection and sensation, nor does it consist in affection alone. Again peripheral excitation fails to account for the active element of conation, while exclusively central excitation overlooks external influence. We must then adopt a theory midway between the two extremes. Wundt, therefore, must be held to state rather the physiological correlate than the psychical fact.

The chief difficulty as to the freedom of the will is found in its connection with the law of causality, which law, however, belongs to the domain of metaphysics, only indeterminism coming within the limits of psychology. *Cause* essentially connotes the inflowing of the agent upon some subject. But *free* and *uncaused* are not synonyms. All action of the will is voluntary, yet not all its action is free. For although the presentation of pleasurable or painful objects to the will, *i. e.*, the motives, together with the agent's temperament and general subjective condition determine the spontaneous impulse of his will, yet it is a fact of conscious experience that he often can and does put forth at the same time an *anti-impulsive* effort. Only actions made under these conditions are rightly called free, and they imply essentially the power to will or not to will.

Yet the law of causality, even in that narrower meaning which obtains in the physical sciences, also applies to free actions in the mass, for we can determine with more or less probability what men taken generally will do under given circumstances. In conclusion, Wundt's assertion that a free act is necessarily an uncaused one, is virtually an admission that the will is superior to material force, and is therefore spiritual.

- (7). *The Consciousness of Identity and So-called Double Consciousness.* By Professor GEORGE T. LADD, Yale University.

The questions in debate concerning the consciousness of identity and so-called double consciousness cannot be intelligently discussed without a critical examination of the conceptions involved. What then do we mean when we speak of a thing, or a mind, as remaining 'identical' or self-same, through various changes of state? To uncritical thought it doubtless seems as though some unchanging 'core' of reality belonged to every being of which we feel ourselves entitled to speak in this way. But philosophical criticism seems rather to assure us only of the proposition: *The real identity of anything consists in this, that its self-activity manifests itself, in all its different relations to other things as conforming to law, or to some immanent idea.*

From this it follows that change, in itself, is not inconsistent with identity being maintained. On the contrary, it is the very character of the actual changes observed or inferred which leads either to the affirmation or to the denial of identity. This principle may be applied to whatever is popularly called a thing, and also to those hypothetical elements of all material things, the so-called atoms.

When we turn to consider the peculiar identity of mind, we find that the affirmation of such identity can never be taken as a denial of change. Indeed, the very real being of mind seems dependent upon change,—in the form, namely, of successive states of consciousness. So that the variety and greatness of the changes experienced *may* heighten rather than diminish the reality and validity of the consciousness of identity, properly described and understood.

Now if we inquire in what consists this conscious identity, we see that it is, and can be, nothing but that which is given to consciousness, in all states of self-consciousness, of recognitive memory, and of reflective thinking about the Self. To have these states of consciousness *is* to be conscious of being identical and self-same. And degrees of the consciousness of identity, as it were, are connected necessarily with all real mental development.

In accordance with this metaphysical analysis we may hopefully, and even confidently, venture upon the attempt to account for the phenomena of so-called double consciousness, in accordance with certain well-known psychological principles. Of these one may be spoken of as the principle of 'psychic automatism.' Under this principle we note in many of our most familiar experiences such a diremption of successive states, or of very complex present states into two-fold combinations of elements, as makes the full impression of *two* interacting personalities, rather than of one person. Yet very subtle and unrecognized or dimly recognized influences of one upon the other, of the Self-conscious Ego upon the automaton, or the reverse, may be distinguished by psychology. All this is popularly expressed either by saying, 'I have the automaton,' or 'the automaton has me;' 'I am the automaton,' or 'the automaton is not me.' Illustrations of all this may be derived from the simpler or more complex bodily operations as under the influence of semi-conscious states, and in turn influencing them; from many deeds of skill and valor, and even of a seemingly high order of intelligence; from the phenomena of artistic and religious inspiration, etc.

Closely akin to this is the most effective working of another principle, which we will call that of a 'dramatic sundering of the Ego.' We can more or less consciously and intentionally, or as forced by circumstances, so 'put ourselves into' another character as virtually to divide the Self into two or more selves, whose appropriate states of consciousness either follow in rapid succession or seem to occur almost simultaneously. The phenomena of dreams, the plays of children, the experience of many actors, the phenomena of certain states of inspiration, the imaginative genius of certain writers, like Balzac notably, are instances in point here. Indeed, the very nature of ethical consciousness, in its highest form of manifestation, necessarily seems to involve such a dramatic sundering of the Ego. In not very infrequent cases, three interacting personalities become manifest in consciousness. These may be described as the

tempter, or bad angel, the good angel, and the Self as the 'torn one,' between the two.

In fine, it seems fair to expect that by a further understanding and more extended application of these, and perhaps other cognate psychological principles, even the most extreme hypnotic cases of so-called double-consciousness may finally be explained.

(8.) *A Preliminary Report on a Research into the Psychology of Imitation.* By Prof. JOSIAH ROYCE, Harvard University.

This report first briefly described a collection of experiments now under way at the Harvard Psychological Laboratory, and then passed to some reflections, suggested by these experiments, relating to the definition of the functions to be grouped together under the name of Imitation. As the text of the report is to appear in THE PSYCHOLOGICAL REVIEW, the present summary need not be extended. The experiments, which at present are only in their first beginning, have thus far been confined to the imitation of somewhat complex series of taps, given by an electric hammer, and arranged in rhythms. The subjects of the experiments imitate the taps, after hearing each rhythm, through repeating the hammer-strokes by means of an electric key. The rhythms, as given and as imitated, are recorded on the kymograph. The effects of habit, in successive imitations of the same rhythm, the influence of speed, and of other factors upon success in imitation, are under study. The complexity of the rhythms studied in these experiments forms one special difference of this enterprise when compared with other experimental studies of rhythm. For the purpose is to study, not the rhythmic consciousness as such, but the imitative functions.

Notes of subjective experiences, taken down during or immediately after each experiment by the subjects concerned, have already given the suggestion for those considerations concerning the definition of imitation with which the major part of the report was taken up,

(9.) *The Classification of Pain.* By Prof. CHARLES A. STRONG, University of Chicago.

This paper was a discussion of the current theory that pleasure and pain are always given as aspects of a content distinct from themselves—the feeling-tone, 'quale,' or aspect theory. It sought to test this theory by considering its application to the case of cutaneous pain.

(1) Neurologically, we know no facts in regard to cutaneous pain which decisively contradict the theory. For special pain-nerves are more than doubtful; and there is a symptom of locomotor ataxia, consisting in hyperalgesia to heat or cold without hyperalgesia to pressure and even with analgesia to pricking and pinching, which seems to prove that some pains are distinctively pains of temperature. The condition of analgesia, moreover, while it implies distinct paths for pain in the spinal cord, may be reconciled with the aspect theory by holding that the sensations called forth through these paths is a tactile or temperature sensation in painful phase.

(2) But, introspectively, it is impossible in certain cases to carry out the analysis for which the aspect theory calls. Extreme pressure, heat and cold produce the same sensation—a sensation not of heat or cold or pressure, but simply of pain. This sensation (*Schmerz*) does not admit of analysis; it is impossible to separate it into a content and an accompanying feeling-tone. But it may call forth an emotional reaction in the shape of a feeling of the disagreeable or intolerable (*Unlust*).

In conclusion, the inference was drawn that pain, being a sensation, may be localized and may leave behind images.

[The paper will be printed in the *PSYCHOLOGICAL REVIEW* for May, 1895.]

(10). *A Theory of Emotions from the Physiological Standpoint.*  
By Prof. G. H. MEAD, University of Chicago.

Prof. Dewey having shown that it is possible to make a complete teleological statement of the emotions along the line of the discharge theory, it is interesting to see how far such a statement may be paralleled by a physiological theory.

This would involve, also, a physiological theory of pleasure and pain. As pain can be differentiated from the sensations in connection with which it generally appears in consciousness, as it shows itself under circumstances in which the tissue of the end organs or the nerves themselves are affected, and as in the diseases in which we find pain as a constant concomitant, those parts are affected, which are richly supplied with blood vessels by means of supporting and nourishing tissues (*Rindfleisch's intermediärer Ernährungsapparat*), and as in those diseases which pass usually without pain (as in the catarrhs of the various mucous membranes) the tissues affected are poorly supplied with such blood vessels, and enter into relation with the capillaries generally through the lymph, for the purposes of secretion, it becomes at least probable that, physiologically, pain may be considered as the interference through poisons or violence or otherwise with the process of nutrition as carried out in the finer arteries and blood vessels. Pleasure must from this standpoint be considered as physiologically the normal or rather heightened process of nutrition in the organs, and the nerve paths which connect these with the central nervous system would be probably the sympathetic.

In the simple instinctive act that lies behind every emotion, the vaso-motor system is called into action by the enlargement of the small blood vessels in the muscles and sweat glands. To maintain the blood pressure the finer blood vessels in the abdominal tracts are closed by the constrictors of that region, and the action of the heart may also be increased by the accelerators. The vaso-motor system thus is, in these simpler instinctive acts, in automatic connection with the senso-motor. The act must commence before the flow of blood can take place. It is in connection with this increased flow of blood that we have to assume the emotional tones of consciousness arise according to the discharge theory. Within the act it would answer only to interest. It is in the preparation for action that we find the qualitatively different emotional tones, and here we find increased flow of blood before the act. We find also what we may term symbolic stimuli, which tend to arouse

the vaso-motor processes that are originally called out only by the instinctive acts. These stimuli in the form in which we can study them, seem to be more or less rhythmical repetitions of those moments in the act itself which call forth especially the vaso-motor response. In this form they are recognized as æsthetic stimuli, and may be best studied in the war and love dances. It is under the influence of stimuli of this general character that the emotional states and their physiological parallels arise. The teleology of these states is that of giving the organism an evaluation of the act before the coördination that leads to the particular reaction has been completed.

- (11). *Desire as the Essence of Pleasure and Pain.* By Dr. D. S. MILLER, Bryn Mawr College.

Pleasure and pain, in the discussion now going forward as to their classification and physical basis, are commonly treated as among our passive sensory experiences; at all events, it would seem to most psychologists a somewhat stupid paradox to assert that they were in any sense motor phenomena. Yet there is solid ground for holding this paradox; for maintaining, at least, that pleasantness (the quality which, along with their specific differences of character, marks all so-called pleasures) and painfulness (the quality which, along with their specific differences of character, marks all so-called pains) are essentially motor facts. A pain is an intolerable feeling; different as they are among themselves, all pains have this, at least, in common, that they are intolerable. No other feeling is intolerable; if it were we should call it a pain. It would, then, not be easy to refute the proposition that painfulness *is* intolerableness; that so-called pains have no other common class-attribute. Now intolerableness is the quality of uniformly provoking a certain bodily disquietude or rebellion, issuing, where the nature of the case permits, in an attempt to escape from the offending irritant. And this is a motor phenomenon. The various *disagreeables* (a term with which 'pains' in my meaning is convertible) a needle-prick, a headache, a burn, the numb internal ache of cold hands, the taste of quinine, the

smell of assafoetida, the scratching of a slate-pencil, 'gnawing pains,' 'shooting pains,' muscular fatigue, disappointment, humiliation—these have no such intrinsic resemblance in sensational complexion as we find among different sights or sounds—between the members of the class of visual, or of the class of auditory sensations; they are similar only in the extrinsic fact that they all alike are accompanied by a bodily reaction—some flinching or shuddering or convulsion, some restiveness or inner tension—which tends then and afterwards to pass into movements of avoidance, escape or repulse. Now these movements and the tendencies to them are what we know as *aversion* in its various forms and degrees.

If painfulness is intolerableness, pleasantness, on similar grounds, is the quality of being *welcome*. The bodily reaction of *gusto* is as characteristic, though not so obtrusive as that of intolerance; and it tends to pass into movements of retention or procurement. These movements and the tendencies to them are what we know as *desire* in its various forms and degrees.

(12.) *Pleasure and Pain Defined.* By Prof. SIDNEY E. MEZES, University of Texas.

It is necessary to find some fact or group of facts that is present whenever we experience pleasure and absent whenever we do not, and another fact or group of facts present and absent with pain. The frequent confusion of unpleasants with pains is very misleading. Unpleasants are of three kinds: memories and expectations, sensational unpleasants that are not pains—bitter tastes, *e. g.*—and sensational unpleasants that are pains—a toothache, *e. g.* We have here to define pleasure and the unpleasant. Attempts have been made to define pleasure-pains as sensations, as emotions, and as making up the genus of which sensations and emotions are two species. The fact that there is evidence for each of the first two theories shows that neither is exhaustive and competent. Besides the existence of pleasant and unpleasant memories, expectations and fancies invalidates all three. Many hold that pleasure-pains are ultimate

ideas, simple and undefinable, like colors. There are strong positive objections to this theory, but negatively, and for our purposes, it suffices that this theory is a last resort, and that its supporters must overthrow all other theories before legitimately claiming it as established. This theory is valuable and true in so far as it points out that neither pleasures as a whole nor unpleasants as a whole have any properties in common. It overlooks the possibility that there may be something invariably co-present with pleasures and some other invariably co-present with pains; and that these two may be the signs to us of the presence of pleasures and pains,—what induces us to call a state pleasant or unpleasant. Now Plato, Aristotle, Hobbes, Kant, and Schopenhauer agree that harmony or good adjustment is the mark of pleasure, ill-adjustment that of pain. Not all these writers point out the terms between which the adjustment is to obtain, but recently Wundt and Ward have held that the adjustment is of attention to its object. This immediately plausible suggestion of attention and adjustment must be examined. Clearly what is not attended to is indifferent since uninteresting. Further immediate attention to pleasures is not the same as that to pains: the former is easy and natural, the latter enforced and obstructed. Again derived attention, always to unpleasants, is invariably obstructed by the more pleasant rivals to attention also present. May it not be that attention without obstruction is the mark of pleasure, attention with obstruction that of pain? The evidence for this view may be thus suggested: All states of intensely concentrated attention are pleasant, hard thinking, hard play, strenuous work; all states of *internal* conflict—hesitation, practical puzzle, co-present irreconcilable impulses, morbidly insistent ideas, etc.—are unpleasant; and further physical pains, owing to their great intensity, reverberate widely and naturally set up mutually obstructive reflexes. The paper appears in the *Philos. Rev.*, Jan., 1895.

- (13.) *Emotions versus Pleasure-Pain*. By Mr. HENRY RUTGERS MARSHALL, New York.

Mr. Marshall reviewed his 'genetic' argument in relation to the Emotions, emphasizing the contention that the

typical Emotions are named because (1°) they correspond to relatively fixed relations between the physical elements reacting, and because (2°) these reactions are immediate. Failure of these two conditions can be traced where 'instinct feelings' have no emotional names. Emotions are in their nature irregular in recurrence, and to be of value must be forceful in reaction; hence Emotions are not usually lost to consciousness as many 'instinct feelings' are, although, if these Emotions become rhythmical and weak, they act as other states do in relation to fixity of habit. Pleasure and pain relate to *organic*, while Emotions relate to *individual or racial*, effectiveness or ineffectiveness; therefore their genesis cannot be considered to have been coincident in time, nor to be of the same type.

The identification of Emotion and Pleasure-Pain in 'Feeling' is dependent upon the validity of the tripartite division of mind; which is upheld by metaphysical postulation but not by psychological evidence. Prof. Croom Robertson argued that the exhaustive categories, The True, The Good, The Beautiful, themselves proved the validity of the division. But the existence of the division is explicable in quite another way, as due to the search for Reality. In relation to mental experience in general, this search gives us the True; in relation to Impression, it gives us the Beautiful; and in relation to Expression, it gives us the Good. If we are to discard this classical tripartite division, we should be able to account for its persistence. It results from an attempt to unify two diverse classifications, both bipartite; viz., 1°, the receptive-reactive classification, and, 2°, the subjective-objective classification: Sensation and Intellect (knowing) being bound together on both the receptive-reactive and on the subjective-objective schemes; Pleasure-Pain and Emotion (feeling) being bound together on the subjective-objective scheme, the receptive-reactive quality being unmarked; Will being marked by a common and coördinate emphasis of the reactive and also of the objective qualities. The existence of this tripartite division, thus explained, can therefore no longer be used as an argument for the bond between Emotion and Pleasure-Pain, which states are dis-

tinctly separable, the relation between them being this: The Emotions are complex psychoses which almost invariably involve repressions or hypernormal activities, either of which are determinants either of pleasure or of pain.

(14.) *Notes on the Experimental Production of Hallucinations and Illusions.* By Prof. W. ROMAINE NEWBOLD, University of Pennsylvania.

Dr. Newbold reported that in 22 out of 86 cases tried he had succeeded in producing illusions by causing the patients to gaze into a transparent or reflecting medium, such as water, glass, and mirrors. His most successful cases were found among young women under twenty years of age who were good visualisers, but as a majority of his subjects were young women, and as the experiments were by preference made upon good visualisers, he was not inclined to lay much stress upon these conditions. The phantasm was usually preceded by cloudiness, flushes of color or of light in the medium, and varied from a dim, colorless outline to a fully developed and brilliantly colored picture. The images were frequently drawn from the patient's recent visual experience, were sometimes fantastic and frequently unrecognised. The successive images were usually associated, if at all, by similarity, but frequently no relation could be discovered between them. Association by contiguity was excessively rare. The phantasm was frequently, but not always, destroyed by movements of the medium and by distracting sensory impressions and motor effort. Occasionally the phantasm was to a considerable degree independent of the medium, persisted for some time after the removal of the medium, and in one such case appeared to obey the laws of the after-image. The importance of such phenomena upon the question as to the value of the central component in the after-image is obvious.

No trace was observed of telepathic or other supposed supernormal agency. There seemed to be no reason for regarding the phantasms of the glass as anything other than illusions of the ordinary types depending upon the glass as a *point de repère*. Their chief speculative importance, apart from the light which they may throw upon the after-image,

lies in the fact that they present to us processes of association by similarity in concrete, sensible form, and in their possible relation to subconscious 'automatic' processes. While the phantasms as such cannot be regarded as demonstrating the existence of such processes, it is probable that, if subconscious automatism exists, its products may be traceable in the phantasms of the glass. It is possible also that some specific relation exists between the hypnotic consciousness and the phantasm of the glass. Dr. Newbold found that images unrecognised by the waking consciousness were sometimes recollected by the patient when hypnotised, and, *vice versa*, experiments by Mr. F. W. H. Myers have shown that a tale related in hypnosis is sometimes presented in the glass externalised in dramatic form.

[This paper is to be printed in full in an early number of the PSYCHOLOGICAL REVIEW.]

(15.) *Experiments on Dermal Pain.* By HAROLD GRIFFING, Ph.D., Columbia College.

By means of an algometer transmitting pressure up to 15 kilog. the average pain threshold was found to be for 40 college students, 5.5; for 38 law students, 7.8; for 98 women, 3.6; for 50 boys, 12-15 years of age, 4.8. The palm of the hand was the place of stimulation. The most sensitive parts of the body are those where the skin is not separated from the bone by muscular and other tissues.

In 80 experiments on two observers the area was variable, areas of 10 mm., 30 mm., 90 mm. and 270 mm. being given. The corresponding average values of the pain threshold were 1.4 kilog., 2.8 kilog., 4.4 kilog. and 6.6 kilog. Thus the pain threshold increases with the area of stimulation, but much more slowly than in direct proportion.

The time in which dermal stimuli of different intensities cause pain was found by noting the time that elapsed before the appearance of pain after weights had been placed in a balance pan in such a way as to press upon the hand. The averages in seconds, based upon 80 experiments on two observers, are as follows: For 100 g., 230 sec.; for 200 g., 35 sec.; for 300 g., 10 sec.; for 500 g., 4.5 sec. Thus the time,

as well as the area and intensity of stimulation, are factors in dermal pain. There is, moreover, an intensive limit below which pressure stimuli never cause pain. Above this limit the sensory effect of the time seems to be in direct proportion to that of intensity.

The pain threshold for falling weights was found to depend as much upon the height as the mass. As both the height and mass are proportional to the kinetic energy of the moving mass, the stimulus for dermal pain in impact must be considered the energy of the striking object.

- (16.) *The Normal Night-Blindness of the Fovea*. By CHRISTINE LADD FRANKLIN, Baltimore.

König's announcement in May, 1894, of the very close coincidence of the curve showing the distribution of brightness along the spectrum for (1) the totally color-blind and (2) the normal eye in a faint light, with the curve of relative absorption of different portions of the spectrum by the visual purple (and the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light are conditioned by the presence of the visual purple in the retina) made necessary some assumption to take account of the fact that no visual purple has hitherto been found in the fovea. Two assumptions were possible,—either that the cones (and hence the fovea) do contain visual purple, but of such an extremely decomposable character that it can never be detected objectively; or, that the eye of the totally color-blind person, and the normal eye in a faint light, are actually blind in the fovea. As I had already made the prediction that total color-blindness consists in a defective development of the cones of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane*, Bd. IV., 1892) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S. III., p. 103)—both predictions being naturally suggested by my theory of light-sensation—I was most anxious to put the latter assumption to the test. I therefore undertook to determine, in the dark rooms of Prof. König's laboratory, the threshold for light-sensation for dif-

ferent parts of the retina and for different kinds of monochromatic light (the full results of this investigation will appear later). The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image *out of* the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident, it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple device of presenting a *group* of small bright objects to the eye of the observer was sufficient to demonstrate the 'normal night-blindness of the fovea' (as it may best be called) without any difficulty,—one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arrangement of a number of small bright spots that the total blindness in the fovea of the totally color-blind boy could be detected,—he had, of course, learned *not* to use his fovea in fixation. Prof. König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue light of wave-length about  $\lambda 470$ .<sup>1</sup> [These experiments upon the normal eye were exhibited at Princeton.] It was shown that König's proof that the pigment-epithelium is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts, and their bearing upon the several theories of light-sensation, were discussed. [This paper appears in full in the PSYCHOLOGICAL REVIEW for March, 1895.]

(18.) *The Muscular Sense und its Location in the Brain Cortex.*  
By Prof. M. ALLEN STARR, New York.

[This paper was presented in the absence of Prof. Starr. It may be found in full in the number of the PSYCHOLOGICAL REVIEW for January, 1895.]

<sup>1</sup> Prof. v. Kries is said to have shown that the experiments in question do not establish the blue-blindness of the fovea (*Berichte der naturforschenden Gesellschaft zu Freiburg*, IX., 2, S. 61). I have not yet had access to this criticism.

(19.) *Psychology in the University of Toronto.* Prof. J. G. HUME, University of Toronto.

In the University of Toronto we begin the work in Psychology, etc., in the Sophomore year. Up to that time the students are engaged in language studies, mathematics, English history, chemistry, biology, etc. After the Sophomore year they still continue some of this language study as supplemental to the philosophical course. The latter, beginning with psychology, logic and theory of knowledge in the second year, psychology, logic, theory of ethics, history of ethics and history of philosophy in the third year, keep extending until, in the fourth year, those who have selected this course give *all* their time to the subjects of the course without any supplemental work, taking, in the fourth year, psychology, ethics, history of philosophy, special reading in the original of various selections from the whole period of modern philosophy, giving special attention to Kant and Lotze.

In experimental psychology: Second year, 2d part of the year: Demonstrations from the Director, explanation of methods and practice. In the third year, during the whole year, the class, divided into groups, is under the charge of the Director of the Laboratory. In the fourth year they are supposed to be able to undertake experiments of an independent character. Some of the inquiries started in the fourth year are continued in post-graduate work.

In the present fourth year there are sixteen honor students conducting four sets of experiments, that is, in four groups, with four in each group: I. On Time reactions (Mechanical registration instead of the Chronoscope); II. Discrimination of Geometrical Figures and Letters in the Field of Indirect Vision; III. Discrimination of Color-saturation; IV. Discrimination and Reproduction of Rhythmic Intervals. In post-graduate study there are two enquiries being continued from last year: I. Estimation of Surface-magnitude; II. On Certain Optical Illusions. The Director of the Laboratory, Dr. August Kirschmann, has in the press a recently finished investigation upon the nature of the perception of *metallic lustre*.

[This paper was presented in the absence of Prof. Hume.]

## DISCUSSION.

### THE SENSATIONS ARE NOT THE EMOTION.

The tendency to assume that the peculiar sensations involved in any psychological fact are the fact itself, comes out strongly in the present discussion over emotion.

When it is shown, for instance, that apart from certain visceral and vaso-motor sensations there is no emotion worth speaking of, we are asked to view emotion and these sensations as identical. Why should we not, quite as well, take emotion to be merely a flutter of thought or a special aspect of attention? Apart from these, there is likewise no 'coarse' emotion.

In fact emotion requires the bodily sensations, but it requires them to be under definite mental conditions which are as indispensable as the sensations themselves. In the first place, some interest which will divert the attention so that these sensations may play the part of mere 'fringe,' is doubtless an important condition for the life of the sensations, but it is also more. Such an interest keeps the sensations in a peculiar relation to the whole mental field. So that the sudden loss of emotion when attention is turned to the body, is probably due less to the fading of the essential sensations than to their reversed relation in the general mental state. Momentarily even during strong excitement, so my observation goes, we can glance at the bodily commotion while many of its most striking elements continue vigorous. We may even cut down between them and us, viewing them as outsiders, as confusion of the body and not of the thinking itself. Instead of strong emotion the state instantly changes to one, say, of psychological query not markedly emotional. The next instant the attention is away, the sensations surge back over the thought, the point of interest is seen *through* the confusion, and the state is unmistakably emotional. As far as I could make out, the sensation-substrate of the two states is about the same, and yet the states themselves are decidedly different.

In emotion we feel that there is confusion in *us*,—in *this* end of the relation. But when we turn upon the bodily sensations themselves, the confusion seems to go over to the other end of the

relation. The object of attention now is in turmoil, but the thought-process itself may for a moment be comparatively calm. The state then need not be emotional, though the object watched is disturbed enough.

It is difficult to give the facts in less figurative language. But substituting the classic figures, we may say that for a state to be emotional it requires a special character of 'form' as well as a special character of 'matter,' whether this matter be taken as sensation or 'tone' or both. In the general upheaval, the operations which relate the sensations are usually more or less disordered. The central nervous processes act spasmodically. Thought is wavering, and the confused bodily sensations seem part and parcel of the confused thinking. But these sensations are by no means equivalent to the emotion. They are merely one abstract aspect of the emotion, of which other important (though likewise abstract) aspects are the rush and whirl of thought, and the special relation of the sensations to the mental field.

LEIPZIG.

GEORGE M. STRATTON.

#### A CORRECTION.

On p. 72 of the last number of the *PSYCHOLOGICAL REVIEW* an omission was made in my abstract of the Sidgwick report on hallucinations which makes the calculated figure of 1300 on line 9 from the top of the page unintelligible. The figure calculated from the premises which I quote is 1400, for which my text substitutes 1300 with no motive assigned. The motive obeyed by the authors of the report is the probable untrustworthiness of accounts of apparitions falling within the first ten years of the informant's life. Such visions are subtracted by the committee both from the total number of recognized apparitions and from the number of coincidental apparitions [See *Proceedings of S. P. R.*, pp. 65, 247]. They form 8 per cent. of the former, so that my abstract of the calculation should have dealt with  $\frac{82}{100}$  of 350 instead of 350. This makes 322, a figure which multiplied by 4 gives 1288. For this the committee substitute 1300, as a 'round number,' slightly more favorable to the adversary.

W. J.

## PSYCHOLOGICAL LITERATURE.

*Philosophical Remains of George Croom Robertson.* With a Memoir.

Edited by A. BAIN and T. WHITTAKER. London, Williams & Norgate, 1894. Pp. XXIV + 481.

In this volume we have, with the exception of the little book on Hobbes and one or two historical articles, all Robertson's philosophical writings. This goodly volume, however, is more than half composed by the republication of shorter articles, critical notices, and notes from *Mind*. An outsider may well wonder why a good deal of this should have been reprinted. The explanation is simple. Robertson held a high reputation in England as a teacher of philosophy. This reputation owed something to the startling and sensational manner in which he appeared on the philosophic scene, when, as a youth of 25, and quite unknown beyond his own Scotch University, he was elected Professor of Philosophy at University College, London, over the head of Dr. James Martineau. This election, due to the strong backing of his teacher Dr. Bain, and Bain's friend George Grote, showed at least that there were some who expected high things of the Aberdeen youth. And their expectations were not disappointed. Robertson proved to be an excellent teacher, endowed with the peculiar gift of guiding the young learner into the labyrinth of philosophic complexities by help of a few well defined clues. To some his lectures were too elementary, and moved too slowly, but to the average student they were exceptionally helpful. He soon began to be known in London society as an authority on philosophical questions. He was a member of the oddly-named Metaphysical Society, the *raison d'être* of which is said to have been the desire of Lord Tennyson, expressed to his faithful attendant, Mr. James Knowles, to ascertain whether he had a soul, though it soon became evident that the experts, viz., the theologians of all creeds and the scientists, who were called in to decide the great question, were much more concerned to attack one another's views. Robertson could hardly have felt quite comfortable here, yet he managed to get this 'metaphysical' *omnium gatherum*, or rather a portion of it, to listen to one or two papers of his own. Outside this society his influence steadily

grew. His appointment to the editorship of *Mind* in 1876, when that journal led the way among English and American philosophical serials, greatly widened the sphere of his influence. This brought him later on into touch with Prof. W. James and other Americans as well as with French and German thinkers. The present given to him by contributors on his retiring from *Mind* two or three years ago showed how warmly he had attached many by his excellent conduct of the journal. For some years his house was the rallying point of the small band of philosophic students of which London could then boast. Leslie Stephen, Shadworth Hodgson, F. Pollock (not then the baronet), F. Gurney, F. Galton, myself, and others, were often to be found there. W. James joined the circle the winter he remained in London. At this time Robertson's talk, which in spite of an occasional smack of the cathedral manner, was distinctly good, gave him prominence in such social gatherings. There was an energy, an alertness, tempered by an Aberdeen 'canniness,' which made him impressive, and he often had a happy way of cutting into a dialectic tangle and extricating the point of real importance. A painful illness was soon to compel him to retire from much of this old social life.

It was necessary to say so much about Robertson's personality as well as his teaching and editorial work in order to explain these *Philosophical Remains*. For a glance at them tells the reader that their collection is the outcome of a feeling of piety. But for this we certainly should not have had reprinted some of the critical notices which in these days of rapid psychological advance already look out of date. The truth is, as he more than once confessed to me, Robertson was not a ready writer. This indeed betrays itself in the literary manner, which, though it has a decided character and certain good qualities, is apt to become awkward even to the point of contortion. The very pains-taking to be clear, to limit a statement to the dimensions of strict accuracy, ended by destroying smoothness.

While there were these half mechanical difficulties in the way of literary production, there was I think another reason for its paucity. There is no evidence that Robertson was ever fully possessed by an impulse to write a considerable philosophical work. The work on which he was supposed, for many years after his appointment at University College, to be engaged, was a study of Hobbes. His little book, which appeared in the 'Philosophical Classics' series, into which the results of these years' study were compressed, shows no doubt careful scholarship, and close critical study of his subject and its historical relations. Yet it does not I think suggest any large

and important originality of thought. It strikes one in reading these *Remains* that Robertson had the freshness of view that goes to make a critical expositor and teacher rather than a true constructive originality. As a teacher he was never so happy as when reading and expounding some philosophic classic to one of his small class of advanced students. The very fact that his one book was mainly a historical exposition seems to say that his bent lay in the direction of philosophic exegesis and of historical criticism. The same impression is, I think, borne out by the *Remains*. The best critical notices seem to me to be those of works on the history of philosophy. Other articles, not dealing directly with the history of the subject, show the same tendencies. Thus the excellent article on 'Axiom,' and in a less degree also the other longer articles reprinted from the *Encyclopedia Britannica*, viz., 'Analysis,' and 'Association of Ideas,' show how Robertson's strength lay in what one may call the expository clarification of ideas. The way in which the word 'Axiom' has come to mean the different things it does is admirably traced out by a happy combination of accurate historical learning and logical co-ordinative power. These papers are in their way models of *Encyclopedia* articles. They show the same qualities, accuracy, perspicacity, grasp, and, what is equally important, a clearly recognizable method, which helped to make him an eminent teacher.

It is time, however, in writing a notice of Robertson's work, for a psychological journal to say something about his work on psychology. As the most distinguished pupil of Alexander Bain, for many years the commanding influence in British psychology, as the hearer of Lotze and other distinguished Europeans, Robertson was always looked on, more than anything else, as a psychologist. And this way of regarding him was in the main justified. He put forward in an admirably clear and convincing manner the claims of psychology to be the propædeutik among philosophers' disciplines. The position is made clear in the introductory lecture which he gave on his appointment to the chair of University College, and is made still clearer in an article on 'Psychology and Philosophy,' published in *Mind*. The establishment of this journal which, as Bain generously allows in his far too short memoir prefixed to the *Remains*, was in considerable part Robertson's work, was intended, as its title and its editorial preface clearly showed, to give fundamental prominence to psychological work and thought, and this intention was never lost sight of. Although there was no experimental psychology in England, and the later experimental work in America had not begun, Robertson managed to get together a good deal of valuable

contribution ; so that *Mind* will long remain an important work of reference for psychologists. His own contributions to the journal show that his mind was fairly engaged with all the newer researches, psychological and physiological, which bear on the understanding of mental processes. Here, however, one recognizes rather the skill with which newer results are brought into relation to older ideas than original contribution, the setting forth of new and luminous psychological ideas. Now and again no doubt there is an attempt to elaborate a new conception, as where in the article on 'Axiom' he seeks to apply the muscular theory of space-consciousness to the problem of mathematical axioms and to show that (as Kant said in his way) by "acting constructively in our experience, both of number and of form, we, in a manner, *make* the ultimate relations of both to be what for us they must be in all circumstances" (p. 129). More ambitious is the attempt to get over the difficulty of the genesis of space-consciousness by saying that we know thing or object as resistant before we know extension or space, that the successions of muscular experience by which we come to know extension, somehow get transformed into the intuitive of space by being referred to the more fundamental object-intuition (p. 279 ff.). It is not quite easy to seize Robertson's exact drift here. Much of what he writes here looks as if he thought the psychologists' task was to explain the objective reference of a space-consciousness already existent, rather than to account for the form or structure of this space-consciousness itself. Yet while these contributions to psychological discussion are not as impressive as one might have expected, they are fresh and suggestive, and they make one regret that Robertson did not give a fuller statement of his views on other perplexing points.

Yet Robertson's friends, at heart, will value this volume as a reflection of the mind and, in some respects, of the character they knew and valued. If it gives us no striking contribution to the field of modern psychological research, it shows us the eager and patient spirit resolved to track ideas to their sources and their elements ; it shows us the born teacher to whom luminous apprehension of truth must express itself in no less luminous an exposition. Such men are as great benefactors as the writers of works. Robertson's devotion to philosophic work, which, as Leslie Stephens' enthusiastic letter tells us, became almost heroic, when for years it had to contend with most unstable health and bouts of prostrating physical suffering, deserves a permanent record in America hardly less than in England.

*Lectures on Human and Animal Psychology.* W. WUNDT. Translated by J. E. Creighton and E. B. Titchener. London, Sonnenschein; New York, Macmillan. 1894. Pp. X+454.

A few years ago a new edition of Professor Wundt's celebrated book *Vorlesungen über Menschen-und Thierseele* appeared. The first edition was written thirty years ago at a time when the problems of empirical psychology had just begun to be realized in all their significance. Since that time psychology as an experimental science has greatly developed; it has adopted and devised exact methods of research; it has followed out carefully many investigations and proved by the results, that the same mathematical accuracy, with which natural sciences like physics and astronomy carry out their work, may be applied successfully to the natural science of mind. The necessity of a psychological way of viewing the facts besides the physical is in our days universally acknowledged. The new edition of the book mentioned is thoroughly revised by the addition of the results of recent investigations and by the omission of every thing which has not stood the test of greater light.<sup>1</sup> In size the book has been considerably reduced, by dropping those discussions which have now developed into a certain independence as special sciences, such as Social Psychology. The book is arranged in thirty lectures, the first thirteen treating Sensation and Presentation. In Lects. 14 to 20, the Feelings and their Relations, the Theories of Association and Apperception are treated. The last part of the book, Lects. 21 to 30, deals with the more complicated problems of animal and human psychical life: Mentality of Higher Animals, Development of Intellectual Functions, Instinctive and Voluntary Actions, Mental Disturbances, Dreams, the Hypnotic and Posthypnotic phenomena, etc., closing with a discussion of the ultimate questions of psychology and their philosophical bearing.

Messrs. Creighton and Titchener are fortunate in having furnished us with a carefully prepared and excellent translation of this book into English; and many who wish to become acquainted with the ideas of this German philosopher, whose efforts have brought about such wonderful advancement of psychological science, will entertain a sense of gratitude to the translators for saving them the trouble of seeking their way directly and in the original language through the more difficult books of the same author.

TORONTO UNIVERSITY.

A. KIRSCHMANN.

<sup>1</sup>A great improvement is also seen in the addition of many good illustrations.

*Primer of Psychology.* By G. T. LADD. New York, Charles Scribner's Sons, 1894. Pp. IX + 224.

Professor Ladd's *Primer* is like his other books in that it is carefully written, systematic, and embodies the latest results of psychological research. It wisely leaves out metaphysical discussion, confining itself to psychology proper, a feature of especial importance in view of the class of readers to which it is primarily addressed. Its style is somewhat unequal, being in parts quite simple and adapted to immature students, and in parts rather burdened with scholastic terms which might, I think, have been avoided. It is not, however, with the use of technical terms that I chiefly quarrel. One may use a rather large number of such terms and yet write in a style which is plain, easy, and entertaining. A book which bears the title of '*Primer*' should be written in such a style. It should be fresh and unstilted, free from all flavor of scholasticism,—it should not smack of the professor's chair. Such books are not easy to write. They can not readily be thrown off as "a recreation between two much more bulky and serious pieces of work." They require great skill, not only in the selection of material, but also in the exposition of the material selected. They call for a rare insight into and a rare sympathy with the ways of thinking of young and immature minds, so easily repelled by what is 'dry,' and discouraged from further effort. Learning may be rather a hindrance than a help in the writing of such books. It may separate one too far from the class of readers one wishes to reach.

In the present instance I cannot but think that the above criticism may with justice be applied. I do not mean to make the criticism at all a severe one, for Professor Ladd's book is usually clear and is well arranged. I should not hesitate to use the '*Primer*' with a class of young people. But I have felt in reading it that it is dry, and that the writer lacks that peculiar gift—a very rare gift it is—of writing successfully for the young. To do this one must above all be fresh and simple and natural. One must forget one's learning, and with it the turns of phrase which are out of place in the '*Elements of Physiological Psychology*' and the '*Introduction to Philosophy*.' When one spends one's life among such, it is, of course, not easy to forget them.

Over the contents of the book I need not linger, as Professor Ladd's opinions are well known. I wish that in the chapter on feeling he had indicated more clearly the ambiguity of the word. Certainly the impossibility of describing what is meant by feeling (p. 53) cannot refer to the complex experiences to which the word is

applied a little later (p. 56). We can at least point out, as Professor James has so well done, some of the elements which enter into these. Such a pointing out of the elements in a complex is what constitutes description of a thing seen, though, in this latter case, the analysis is one more readily made. Again, it does not seem to me likely to aid one in clearing up the psychology of memory to state that "every act of memory with recognition transcends the present, and connects the present into a known real unity with the past;" and, having thus stared the difficulty boldly in the face, to pass on with the remark that this is one of the profoundest of all mysteries. The mystery is, I think, not psychological, but, if it exist at all, epistemological. In psychology we are concerned only with the question, "What mental elements are actually present in a given mind when it recognizes something?" These elements we may not be able to enumerate, and in so far we may call them mysterious; but when the problem is stated psychologically it does not, I think, present so hopeless an aspect as it does when stated as it is by Professor Ladd.

In looking over the above I find I have made my criticism more negative than I had intended. I have not dwelt upon the merits of the 'Primer' as much as I have upon what appear to me its shortcomings. It was perhaps as well to do this, for it goes without saying that a new book by Professor Ladd should have the strong points which characterize his other books. The author has been too long in the field, and is too well known, to make it necessary to praise him.

G. S. F.

*The Elements of Metaphysics.* PAUL DEUSSEN. Translated from the Second Edition by C. M. Duff. London and New York, Macmillan & Co. Pp. XXIV + 337.

Dr. Deussen, the author of the *Elements*, has been known for years as an enthusiastic student of Indian Philosophy and a representative of that school of Orientalists who reject the negative conceptions that have been historically associated with Buddhism and follow Caṅkara as the true interpreter of Hindu Metaphysics. Dr. Deussen's own philosophical position is Kanto-Schopenhauerian. Kant he asserts was the first discoverer of the true principle of philosophy, while Schopenhauer alone has developed that principle truly and said the last word in metaphysics. This being the author's faith his work is on the whole a pretty faithful reproduction of the philosophy of these masters.

Dr. Deussen starts with a distinction between Science and Philosophy. The standpoint of science is empirical and materialistic. Materialism can be overcome only in the transcendental standpoint of philosophy which regards the world under the dual Kantian categories of phenomena and things in themselves. This Kantian distinction has been translated by Schopenhauer into corresponding subjective terms, intellect or reason, and will. The world is dual, it is a world of intellect and a world of will. Now, the intellect whose innate forms are space, time and causality is a purely phenomenal faculty through which the will projects a world in space and time and causal connection. But this world is appearance and not reality. The intellect is material in its objective constitution, being identical with the brain. But this whole world of the intellect is appearance and must be transcended in order that the world of reality may be reached.

The thing in itself, or real, is the will whose central motive is the striving for life or self-realization. This striving of will expresses itself as the Platonic ideas in the physical forces of the world, thus grounding the phenomenal world in its deeper dynamic aspect. Dr. Deussen makes a tripartite division of transcendental philosophy into the metaphysics of nature, of the beautiful, and of morality. The main ideas of the first division have been given above. In nature which also includes the ordinary phenomena of man, the will does not manifest its archetypes as they are in themselves, in their 'unspoiled form and beauty,' but only an adumbration of these. For the more adequate expression we must pass *first* to the Metaphysics of the Beautiful and *finally* to the Metaphysics of Morality. Nature is the expression of the affirmation of the will to life which is empirical, individual, egoistic. It can be transcended only by *denial* through which alone is a door opened into the heart of reality. Now, art in its feeling for the beautiful which Kant defines as a 'disinterested delight,' enters this door through a kind of self-forgetting of the will. The will is affirmative in its nature and does not care for things in themselves, but only as they affect it. But in the art-feeling this egoism drops temporarily out of sight and the will experiences a delight in that which has no reference to itself. This is a contradiction which art cannot explain and we are led on to morality for its solution. It is only in morality and religion that the phenomenal world and its contradictions are actually transcended.

Dr. Deussen's treatment of the metaphysics of morality embraces the following essential points. (1) The tripartite classification of the will-functions under the dual categories, Physics and Metaphysics,

giving the following:—*Thinking* as empirical and transcendental; *Perceiving* as individual and æsthetic; *Acting* as affirming and denying; the metaphysical exercise of these functions giving respectively, Philosophy, Art, Religion. (2) The principle of sin and evil which is egoistic affirmation. This is the root of both sin and suffering. (3) The Principle of Morality which is *denial*. Salvation from sin and suffering only comes through the denial of the will to life. (4) The way of achieving this self-denial of will. This includes, (a) the classification of springs of action arising from *Affirmation* and *Denial*; from the former *malice* and *egoism*, representing Paganisms; from the latter, *compassion* and *asceticism*, being the dominating motives of Christianity; (b) the two paths to self-denial, sympathy and suffering; (c) also the steps by which denial is achieved, justice, love, asceticism. (5) The goal of morality which may be expressed in various ways as the Kingdom of Heaven, Blessedness, Peace that passeth understanding, Nirvana. This state, as Deussen conceives it, is reached by a transcendence of individuality, but it is not purely negative. It is a state of positive experience and the denial itself cannot, therefore, be absolute.

Here, I think, we strike a crucial point in the metaphysics which Deussen represents. It seeks to make denial the last word in religious philosophy. But it manifestly is not the last word if the goal, the kingdom of heaven, nirvana, is not purely negative. In morality we strike a dualism between a lower and a higher self, as a basal fact. In the light of this, egoism becomes the affirmation of the lower self. But in its relation to the higher self it is denial and what the metaphysics which Deussen represents, calls denial, is in truth the *denial of denial* and is thus a higher affirmation. I do not see how the last word of morality and religion can be anything else than affirmation, an affirmation in which the highest self is realized.

From the psychological point of view Deussen's book possesses several points of interest. In common with the writers of his school he has done service to psychology in the emphasis he places upon the will. But just here, I think, we strike the greatest psychological defect of the school; its tendency to divorce too completely the intellect from the will. The inevitable result of this is a shallow conception of the intellect on the one hand and the identification of will with blind instinct, on the other. Between the two the teleological character of consciousness is lost sight of or inadequately treated. Again while Dr. Deussen's work is rich in fragments of psychological analysis, it is almost totally lacking in dynamic and genetic conceptions. Perhaps this is an unfair criticism to make on a work in met-

aphysics. But I think our whole metaphysical conception of the relation of phenomena to an absolute ground, or, in Kantian phrase, to things in themselves, will be profoundly affected by our psychological faith on this point. If we admit the genetic idea in psychology our whole world will become impenetrated with dynamism and it will no longer be possible to treat the phenomenal as mere appearance.

But enough of criticism. Dr. Deussen's metaphysics is one of the most valuable of the many works that have been appearing lately in English. And its English dress is in every way worthy of it and creditable to the translator. It is a book that no one can read seriously without getting rich suggestions and having his spiritual intuition greatly quickened. And the fine religious spirit that pervades it will commend it to every one who values the religious aspect of philosophy.

A. T. ORMOND.

PRINCETON.

#### ETHICAL.

*The Elements of Ethics.* J. H. HYSLOP. New York, Charles Scribner's Sons, 1895. Pp. VII., 476.

Dr. Hyslop's book might appropriately be entitled an analytic of Logical Conceptions. Its purpose is critical and analytic rather than constructive. The work has been done with that thoroughness and detail which we would expect from a man of Dr. Hyslop's ability and logical equipment.

After defining ethics and considering the sense in which it is a science, and its relations to other sciences, in his introduction, the author gives in Chapter II a very interesting digest of the history of ethical thought in ancient, mediæval and modern times down to Hume. This is valuable in itself and a very good Introduction to the discussions which follow. Chapter III is devoted to defining terms and stating and defining the elements entering into the ethical problem. In chapter IV on the Freedom of the Will, the most conspicuously able chapter in the book, Dr. Hyslop distinguishes the various species of freedom, identifies moral freedom with *velleity* or power of alternative choice, defends it against necessitarian objection and shows how man's physiological mechanism, through its function of inhibition, adapts him to the exercise of free choice and volition. The bearing of heredity and environment and the general question of motives are treated with ability and discrimination. Dr. Hyslop rejects indeterminism and identifies freedomism with the

species of determinism which recognizes the possibility of alternative choice. The discussion on freedom prepares the way for a discriminating analysis of Responsibility and Punishment in Chapter V. Chapters VI and VII treat of the Nature and Origin of Conscience. The analysis of conscience is interesting but need not delay us. In his chapter on the Origin of Conscience Dr. Hyslop discusses nativism and empiricism, and their various subdivisions. He is a nativist and yet gives generous recognition to empiricism in both its individualistic and evolutionary forms. Nativistic theories include three species, Theism, Naturalism and Intuitionism. Distinguishing two forms of Intuitionism, general and special, Dr. Hyslop accepts the former which affirms an original power distinguishing right and wrong, but not the right and wrong of particular acts, as the theory that is best borne out by the facts. This enables him to admit the claims of empiricism to a large extent and to recognize a wide sphere for evolution in developing morality out of its elements. The only concept of evolution which Dr. Hyslop rejects is that which ascribes to it a creative function and claims that morality can be developed out of conditions that contain none of it. From this point of view the theories of Darwin and Spencer are criticised and the position is combatted that the theory of evolution necessitates any radical reconstruction of ethical theory. In Chapter VIII, theories of the nature of morality are classified and discussed. Adopting a classification based on the *end* or *summum bonum*, Dr. Hyslop divides theories generally into Hedonism and Moralism. The Hedonistic theories agree in making pleasure or happiness the moral end and divide into Egoistic Hedonism and Utilitarianism. Moralism sets either excellence or duty as the end and this divides into Perfectionism and Formalism or, as some writers call it, Rigorism. A careful analysis of these conceptions of the end reveals elements of value in them all and supplies the data for a more adequate synthesis. The last two chapters, IX and X, discuss the important topics, Morality and Religion, and the Theory of Rights and Duties.

The merits of Dr. Hyslop's book are so great as to make criticism seem almost impertinent. I venture, however, to note several points on which I think a little more explicitness would be desirable. *First*, regarding freedom. Dr. Hyslop appears to limit the power of alternative choice to means. But is there not also a choice of ends, and is not this more of the essence of freedom than the mere choice of means? If our nature determines our end must it not supply dual alternatives for choice between higher and lower ends? Again, gradations of freedom are recognized and Dr. Hyslop

is willing to admit that some people may have very little of it. Does he mean velleity, and if so must he not recognize a more vital connection between heredity and freedom than he seems willing to admit. *Secondly*, it seems to me, and will perhaps strike others in the same way, that Dr. Hyslop has carried the legitimate distinction between the questions of origin and validity so far as to lose sight, partially, of the vital bearing which theories of origin must have on questions of validity. In spite of the logical separation, our psychogeny will determine largely the complexion of our metaphysics. And in this connection it seems to me that evolution has a more vital relation to ethical theory than Dr. Hyslop allows to it in his discussion. *Lastly*, respecting the relation of morality and religion, while agreeing with Dr. Hyslop's major proposition that the validity of moral science is not to be staked on the acceptance of any religious postulate, and with nearly all that is said in connection with it, I still feel that the religious thinker will have some grounds, in view of the whole discussion, for thinking that the problem has not been treated with sufficient insight. He will be likely to think that religion has been pushed a little too much to one side, and that just as it is possible to recognize the full right and independence of science in the sphere of its own categories, and yet to subsume it under the categories of metaphysics, so in the case of religion it is possible to recognize the full right and independence of moral science, while at the same time subsuming it under the categories of religion. The religious thinker will be disposed to regard the relation as one rather of comprehension and harmony, than of exclusion and mutual conflict.

But the faults of Dr. Hyslop's book are few compared with its merits. I feel under a great debt of personal obligation to the author for his masterly and luminous analysis. For the task that Dr. Hyslop has performed there was great need in this country and the work has been thoroughly done. His book is the most notable of recent contributions to the science, and will give him a front rank among ethical thinkers. It will also tend to raise the plane of ethical discussion in this country, and to put the problems of morality in a position where they can be more sharply defined and more intelligently treated.

A. T. ORMOND.

PRINCETON.

*Johnson's Universal Cyclopædia.* Vols. I-V (A—Mozambique). New York, Johnson Co., 1894.

The striking feature of the philosophic content of this Cyclopædia compared, not simply with former editions of itself, but also with

other cyclopædias, is the much more adequate attention given to psychological topics. This may not unreasonably be attributed, I suppose, to the presence among its editors of Professor Baldwin; just as the editorial care of Dr. Harris had previously made the metaphysical side of philosophy more prominent in Johnson's than in any other save the Britannica. There seems to be particularly good reason for ascribing the difference to the interest of Prof. Baldwin in the fact that it is only in the fourth and fifth volumes, after Prof. Baldwin is well installed in the editorial chair, that the psychological articles become numerous. Some of the psychological topics which are so unfortunate as to begin with *A* or *B*, are in quite striking contrast to the accuracy and fullness of the later articles. The article on Association of Ideas, for example, gives a fair descriptive statement, but is quite innocent of modern problems and methods, to say nothing of results. In contrast with the definitely experimental tone pervading the later articles, it is somewhat startling to read regarding association, that the search for a physiological solution is in vain, and to find the following proposition set forth as an explanation: "This wonderful power of the human mind is part of the perfection which it owes to the Great Being who is its author."

The letters *G*, *H* and *I* are fortunately very rich in psychological captions and the comparative barrenness of the earlier pages is more than made good. I know of no better way to give an idea of the variety of topics treated than to give a running list of the more important subjects: Genetic Psychology, Genius, Habit, Hedonism, Hypnotism, Ideal Feelings (Emotion), Ideals, Illusion, Imagination, Imitation, Impulse, Innervation, Instinct, and Insanity, all by Professor Baldwin; Generalization, Hegel, Hindu Philosophy, Idea, Idealism, Identity, Immortality, Infinite, by Dr. Harris, and Intuitionism, by the present writer. The article on Histology by Dr. Piersol should also be mentioned. In general, it may be noted that the neurological side is quite carefully looked after.

To go into as much detail regarding all the letters would render this notice a catalogue, not a review, but the articles on Localization (in space and of brain functions) and upon Motive by Professor Baldwin, and that by Dr. Cattell upon memory should be noted. It is in no way invidious to any of the other articles to say that the article upon memory is in respect to its objectivity, lucidity and presentation of current scientific problems and method, a model of what cyclopædia information should be.

Several of Professor Baldwin's articles seem to me a distinct advance upon his own statement of the same subject in his Psychology.

The idea is more definitely put, and the style more precise. There are many of these articles to which not only the 'general reader,' but the psychological specialist will turn with interest, and, judging from my own case (if I may venture for the nonce to pose as a specialist) with profit. The article, for example, on impulse is highly suggestive; the reference of impulse to the central apparatus as representing the growth of the whole system, rather than to a specific stimulus, appears to be a very decided advance upon previous efforts to discriminate impulse from reflex-action. The article upon imitation is excellent, as we should expect from one who has made the psychology of that subject peculiarly his own. The article upon emotion (under the caption of Ideal Feeling) is admirable, save the attempt to state the theories offered in explanation. Of course not everything can be given in such an account; and yet surely the contribution of James-Lange is too important, whether accepted or rejected, to be so briefly summed up. The attempt of Darwin to explain emotional expressions might well have received some attention. The article on Imagination would have been helped by reference to the concrete investigations in imagery; but aside from that it is well done. (There is a heading Generic Image, referring one to image, but the latter does not appear as a distinct topic; it may also be noted in this connection that a *q. v.* to Insistent Ideas is found in the article upon Illusion, but no such caption occurs.) The article upon Genetic Psychology is too short to give Prof. Baldwin a fair opportunity, but fortunately we shall soon have a chance to read a fuller expression of his views. This present account is clear and full within its limits. But I wonder when I read the following: "Suppose we say, with many psychologists, that volition is necessary to all adaptive muscular efforts; an appeal to the child shows us so many facts to the contrary that we are able to bring genetic psychology to refute the position." I do not wonder at Prof. Baldwin's saying this; on the contrary, it is true enough to immediate facts. But I wonder if the final outcome of the appeal to the child will not be to change the ready-made concept of volition which serves as the standard in the above instance, and to generalize the idea of volition by making it equivalent to all acquired coördination.

However, I might go on indefinitely commenting upon points of interest. I shall fulfill my duty better if I divert the attention both of psychologists and the general public to the unusually full and suggestive discussion of psychological topics to be found in this last edition of Johnson's cyclopædia. Teachers will find its great value for reference further increased by the generally good and up-to-date bibliographies.

J. D.

## EVOLUTION AND BIOLOGY.

*From the Greeks to Darwin.* H. F. OSBORN. Columbia University Biological Series, No. 1. New York and London. Macmillan, 1894. Pp. X+259.

*Amphioxus and the Ancestry of the Vertebrates.* A. WILLEY. Columbia University Biological Series, No. 2. New York and London. Macmillan, 1894. Pp. XIV+316.

In these two volumes we have the beginning of a biological series which promises to be of importance for psychologists, since the topics of these and other volumes announced are the broader and more philosophical ones in the settlement of which the theory of the mental life is also involved. Prof. Osborn traces the history of the evolution idea before Darwin in an interesting way and with great perspicuity of style. From the psychologist's point of view more reference to mental development might possibly have been made; and yet it may be that the author found that his intimations of Darwinism before Darwin were not capable of such a reference. The book of Dr. Willey deals with the very vital question of the ancestry of the vertebrates; and while the conclusions upon the broader matters of descent are not large, still psychologists should know many more facts than they do of just the sifted kind which are here given. Our space only allows us to recommend these books, not on our own authority indeed, but as already approved by the biological authorities to whom we must defer.

*The Factors in Organic Evolution.* D. S. JORDAN. Boston, Ginn & Co., 1894. Pp. V+149.

It is difficult to see what purpose this volume can serve. Dr. Jordan prints a great mass of catch-sentences, clauses, and words under the main headings of current evolution thought, sometimes calling upon his colleagues to treat special topics in the same brief and unsuggestive way. It is possible, of course, that the author may find such a 'syllabus' useful in the hands of his classes, while he himself fills out the outline by lecturing. But why he should publish it—why? Those readers who know what the terms and catch-words mean, and know intelligently, do not need to be reminded of the categories of the subject; and those who do not, are not taught. Possibly a few teachers who lack time to plan their own lectures may follow the author's skeleton. The peculiar way of printing the book with double blank pages throughout would seem to indicate that this is the writer's idea. But time-limits and sense-limits in different schools and colleges are so different that independent men will probably prefer to do their own schematization.

*Illustrated Dictionary of Medicine, Biology, and Allied Sciences.* G. M. GOULD. Philadelphia, Blakiston, 1894. Pp. XV+1633.

The 'Dictionary' falls midway between two others which psychologists, who can afford them all, ought to have: One is Tuke's *Dictionary of Psychological Medicine*, and the other is Quain's *Medical Dictionary* (2d ed. 1894). Gould's book is more properly a dictionary, while the others are more properly encyclopedias. As a dictionary, Gould's work defines, in a reliable way, the terms of the whole group of cognate branches which touch upon biology and ought to serve a very useful purpose to psychologists, especially in these days when pathology, on one side, and development on the other, are bringing medicine and biology into such close touch with our own proper study. The present reviewer is not competent to criticise the definitions except as they are in his field; but judging from the sample topics in which he feels at home, and from the auspices under which the work appears, it seems altogether reliable. It is a pity more terms were not drawn from psychology for the benefit of the physicians and biologists who will be the main buyers; for they need instruction in general psychology.

*Epitome of the Synthetic Philosophy.* F. H. COLLINS. London, Williams and Norgate, 1894. 3d ed. Pp. XIX+639.

The original 'epitome' by Mr. Collins is now so well known that we need only call attention to this late edition. Its advance on the earlier editions is of course apparent, since it includes Mr. Spencer's last publications. The compiler indicates just what the addition is in these words: "By Mr. Spencer's kind permission I am enabled to include in this edition an abridgment to one-tenth (the proportion which holds all the way through) of his recent *Principles of Ethics*. The present volume thus represents in miniature the whole of *The Synthetic Philosophy* at present published."

J. M. B.

#### CHILD PSYCHOLOGY.

*Notes on the Development of a Child.* M. W. SHINN. Part II. University of California Studies. Vol. I, Berkeley, 1894. Pp. 89-178.

This is the second installment of Miss Shinn's valuable observations on her little niece. The paging is continuous through the two parts, making a total so far of 178 large pamphlet pages. The notes on the sense of sight are brought to a close by a chapter on 'Sight in the Third Year,' then Hearing, the Dermal Senses, Taste

and Smell are treated. By the end of the second year the discrimination of colors was practically complete, but no æsthetic interest in color-arrangements was apparent even in the third year. The child seemed, however, to be conscious of the defects in her attempts at drawing. Sensitiveness to sounds seemed different at different times, even apart from the effects of fatigue. Her attention was attracted by the experience of 'double touch' (the hands touching each other) on the 64th day. Sensibility to pain through hurts or extremes of temperature was very low during the first two months. "Taste at no time played as large a part among the child's interests," as the author expected. "To see others eating a favorite food was often desired as a substitute for eating it herself." If this comparative indifference to tastes was due (as the author seems to think) largely to training, it seems to me that a point is suggested here of the very highest pedagogical importance.

*De la suggestibilité naturelle chez les enfants.* A. BINET and V. HENRI.  
Revue Philosophique, Oct., 1894. P. 338.

Understanding by 'natural suggestion' that form of influence which, in ordinary conditions, people exercise upon one another, the problem was to investigate the effects of natural suggestion upon the simple judgments of children. The children were graded according to age, as follows: 1st grade, children between seven and nine; 2d grade, children between nine and eleven; 3d grade, children between eleven and thirteen. The tests involved suggestions of three sorts:

(I) *The suggestion of a preconceived idea.*—Three lines of different lengths were shown to the child in succession; then, after a short interval, a chart was presented to him containing lines varying regularly in length, of which Nos. 5, 11 and 18 corresponded in length to the three models; and he was asked to pick out those lines which were equal to the models. Then the test was repeated with this difference: that in the chart now presented to the child the five longest lines (Nos. 17-21), and therefore the line corresponding to the third model were lacking. The force of suggestion would be felt in the expectation, on the child's part, of a uniform experience in the two tests (especially as in the second case the third model was shown him, just as in the first), and in his natural timidity and reluctance to declare the absence of the looked-for line, even if he suspected it. Over against this we must place the accuracy of his judgment and the correctness of his memory. The result was that a certain number believed themselves to find in the second chart a line of the length of the third model, and chose accordingly. Now

of these a certain number had in the first test chosen for the third model a line below No. 16 on the chart of 21 lines. These were now counted out, for the obvious reason that since they had made this erroneous judgment, where there was no suggestion, they could not be presumed to have made it *through* suggestion in the second case. The whole experiment was now repeated with the rest, with the result that the susceptibility to suggestion *was, on the average, in inverse proportion to the child's age.* Of the children in the first grade, 88 per cent., in the second 60 per cent., and in the third 47 per cent. yielded to the suggestion of the preconceived idea and selected a line for the third model from the second or incomplete chart. So far we have only judgments based upon memory. Now the child was allowed to see the model and the chart at the same time (direct comparison). Using only one model (the third) the results were: with the complete chart 67 per cent. of the children erred in direct comparison and 79 per cent. in memory comparison. When the 16-line chart was used, 38 per cent. made errors in direct comparison and 65 per cent. in memory comparison. There seemed also, in the case of direct comparison, more assurance and less timidity, and so less susceptibility to suggestion, than in the other case.

(II) *Verbal Suggestion.*—A line 40 mm. long was shown to the child, and he was asked to choose a line of that length from a chart as before. At the moment of his doing so, however, the experimenter said, in a calm, even voice, and without gesture: 'Are you quite sure you are right?' The result was that, in the case of memory-comparison, 89 per cent. of the 1st grade children, 80 per cent. of the 2nd grade, and 54 per cent. of the 3rd grade hesitated and then *changed their selection*, under the influence of the verbal suggestion. In the case of direct comparison the figures are: 1st grade, 74 per cent.; 2nd grade, 73 per cent.; 3rd grade, 48 per cent. Here, again, the younger the child the less stable his judgment and the more open he is to suggestion. Here, too, we see as before that fewer errors are made in direct comparison than in memory-comparison. Again it was observed that those whose judgments were correct were less open to suggestion than the others. Only 56 per cent. of the former to 88 per cent. of the latter changed their selections. Again, of those who changed their selection on account of the suggestion, 81 per cent. changed it *for the better* (i. e., for one more nearly correct), only 19 per cent. changed for the worse. This is surprising, especially in the case of memory-comparisons: one would expect that the interruption would have made the child nervous, and so hindered instead of helping his judgment.

(III) *Suggestion in collective experiments.*—Taking four pupils at a time, they are allowed to see the model line and the chart of lines simultaneously, and then are asked to say, all at once, which line is equal to the model. Generally they do not all answer together, and the slow ones come under the suggestion of the quicker ones. In such cases there is a surprising uniformity among the younger children, the older pupils being more independent in their answers. Yet in all the grades there is a great susceptibility to this sort of suggestion. The percentage of correct answers was somewhat higher, when taken in this collective way, than when taken individually (34 per cent. to 23 per cent.).

*A Preliminary Study of Motor Ability.* J. A. HANCOCK, Pedagogical Seminary, Oct., 1894.

This study by Mr. Hancock well deserves the careful study of all interested in child education. A large number of school children from five to seven years of age were tested in various ways to discover the amount of muscular control they possessed. For full description of the tests and apparatus and detailed statements of the results the original article must be consulted. The results can be stated here only partially and in a general way, *e. g.* :

I. A child cannot stand so still as a man. Men swayed, on the average, 3.5 cm. in the antero-posterior direction and 2 cm. laterally. Boys of six years swayed 5.1 cm. by 4.3 cm.

II. A child cannot hold his hand so still as a man. Men moved their hands on the average .242 cm. by .752 cm.; boys of six moved theirs 1.191 cm. by 4.258 cm.

III. A child cannot hold his attention upon any subject so steadily as a man. Some of these experiments, which required sustained attention for one minute, could not be carried out in the case of the children.

IV. Control of the arm is far greater in men than in children. With the former the 'Trenograph' registered an average movement of .0975 cm.; with the latter of .396 cm.

V. The child cannot *tap* so rapidly as the adult. The rate for the child of 16 years is five times as great as for one of six years.

VI. The order of control is from fundamental to accessory muscles (*i. e.*, larger muscles come under control earlier than smaller). Fine, complicated movements are difficult for the child.

VII. The prolonged effort to keep quiet produces in children strong symptoms of nervous irritation.

VIII. "Generally the girl, at the same age, is steadier than the boy."

IX. "Children in normal healthy growth show a lack of coördination and control paralleled only by ataxic, choreic and paralytic patients."

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#### NEUROLOGY.

*On the Inadequacy of the Cellular Theory of Development and on the Early Development of Nerves, particularly the Third Nerve, and of the Sympathetic in Elasmobranchii.* ADAM SEDGWICK. The Quarterly Journal of Microscopical Science, N. S., No. 145, November, 1894.

During the past decade the protest has been coming from many quarters and with increasing strength against certain crude notions which, as accessories, have attached themselves to the cell doctrine.

Our author points out the condition of affairs, and summarises the usual training in these dogmas with telling effect. He then proceeds to illustrate the darkness of this biological age by several examples. During his various studies, especially those on *Peripatus*, he has been struck by the fact that in many cases tissues refused to break up into cells or cell layers. "It would appear," he says, "that in *Peripatus* the cells of the adults in so far as they are distinct and sharply marked off structures, are not, as appears to be generally the case, present in the earliest embryonic stages, but are gradually evolved as development proceeds. In other words, the cell theory, if it implies that the adult cells are derived from embryonic cells which have been directly produced by the division of the ovicell, does not apply to the embryos of *Peripatus*." For further illustration Sedgwick then takes up the so-called mesenchyme tissue of *Elasmobranch* embryos; the origin of nerve trunks and the fate of the neural crest.

The ideas fundamental to the view urged by our author are set forth in what is stated concerning nerve cells. According to this hypothesis nerve fibres are present before the nuclei or cell bodies appear. The principal function of the neural crest, so far as it takes part in forming the nervous system, is to produce nuclei which ultimately attach themselves at various points to this reticulum from which the fibres are formed by condensation. The details need not be given, for it is at once evident how very widely such a view differs from the one current, and according to which the nervous system takes origin from a series of spherical cells which later produce

the fibres as outgrowths. This new explanation certainly deals with but a fraction of the facts. Most anatomists are ready to admit that in the matter of cell formation as in other life processes there are wide variations, all the way from distinctly marked cell elements as disconnected as blood corpuscles through forms incompletely separated, to those in which nuclei appear scattered in a poorly divided enclosing mass. That any one of these arrangements should be chosen as representing the 'whole truth' and the others 'reduced' to it is more likely to hamper than to enlarge knowledge. It is psychologically interesting that we glory in the thought of transformation and genetic evolution, yet dominated by the notion of types and the mistaken idea that profound conceptions must be capable of simple expression, in the same breath utter a partial hypothesis which is warranted complete, and thereupon proceed to 'whip in' the non-conforming facts. There is but one excuse for this, namely, that each hypothesis must be pushed in every direction in order to demonstrate its truth or falsity, but we rely on our colleagues to exercise good judgment in the process and not mislead us, for grave responsibilities attach even to the exercise of the scientific imagination.

*A description of the cerebral convolutions of the Chimpanzee known as "Sally"; with notes on the convolutions of other Chimpanzees and of two Orangs.* W. BENHAM. The Quarterly Journal of Microscopical Science, N. S., No. 145, November, 1894. Plates 7-11.

Chimpanzee brains differ among themselves and the zoölogists hint at two or even three species of this animal. The individual 'Sally' who lived eight years at the Zoölogical Gardens in London has been referred by Beddard to the species *Troglodytes calvus*. The brain from this case is in some respects unsimian in its conformation.

In the majority of cases the chimpanzee brain possesses an occipital operculum, a distinctly simian feature. This was quite absent in the case of Sally. Further the demarkation of the insula and the branches at the anterior end of the Sylvian fissure were more than usually evident. Thus this specimen serves to diminish in various important characters the differences in form between the brain of man and of the chimpanzee as generally described. The accompanying plates are excellent.

The two other chimpanzee brains most similar to that of 'Sally' have been described by Broca and by Müller respectively. Both the latter brains were from young males, the species not having been exactly recorded. Hence there is no positive evidence that this type

of brain is characteristic of the species *calvus*, but at the same time it is not to be correlated with either age or sex.

*Amusie (Musikalische Aphasie.)* J. G. EDGREN. Deutsche Zeitschrift für Nervenheilkunde, B. VI. H. 1-2. December, 1894.

The perception and expression of musical sounds and symbols can be shown to be quite parallel to that for the sounds and symbols of ordinary speech. Beyond the musical faculty comes gesture language, which is a form of expression even more general than music. It has occurred to Ballet to picture the three brain areas concerned, as three concentric circles, of which that representing verbal speech should be the smallest and that for the emotional gesture language, the largest; the musical faculty falling between the two. Against such a scheme there are many important objections; but it serves to emphasize the fact that in any one instance the anatomical bases for the reactions are not identical with those in the others, although several structures may be used in common. In the study of aphasia, the disturbance in the musical faculty has been neither generally tested nor recorded in detail. E. is able to find in the literature, which he summarizes with great skill, a number of cases of aphasia without amusia; another group in which they are combined, and a third group, in which the amusia in one form or another, is alone present.

The impulse to his study of this subject was a case of amusia in which both the clinical history and record of the autopsy were at hand, and in which the brain lesion in the left hemisphere was a destruction of the anterior two thirds of the first temporal, and the anterior half of the second temporal gyrus, together with destruction in the right hemisphere of the middle and posterior portions of the first temporal gyrus, and the ventral edge of the inferior parietal lobule along the Sylvian fissure. Both lesions are shown in figures.

In general the author concludes that the musical faculty like that of speech can be disturbed by lesions of the brain. The different forms of amusia are comparable with the different forms of aphasia. These are clinically distinct, and while the analogous forms of aphasia and amusia may occur together, they are not necessarily associated. There appears also to be a distinct anatomical basis for the forms of amusia as contrasted with those for aphasia and for that form of amusia designated as note-deafness (his own case), there is some reason to locate the cortical centre in the first and second temporal gyri of the left hemisphere, somewhat in front of the region, injury to which causes word deafness.

*Recherches microscopiche e sperimentali su gli effetti della Tiroidectomia.*

F. CAPOBIANCO. Internationale Monatschrift für Anatomie und Physiologie, Band XI-H., II and XII, 1894.

There is often a tendency to overlook the importance of nutritive conditions in modifying the reactions of the nervous system. It has been shown, however, that there exists a close connection between the thyroid body and the central nervous system. On the pathological side the various forms of goitre, dependent on changes in the gland, and associated with disturbance of the nervous functions, give still further support to this idea, and Foster remarks in his text-book of physiology that the senescence of the nervous system is probably involved in the early atrophy of the thymus.

The work of Capobianco touches two points; the general effect of the removal of this gland from dogs and rabbits, and the changes which at the same time occur in the nervous system. As a result of the total extirpation of the thyroid gland, dogs and rabbits always die within four weeks, the average life of the rabbits being longer than that of the dogs.

Histological examination of the nervous system, central and peripheral, showed distinct pathological changes in cell-bodies and in the fibres together with alterations in the blood-vessels. In dogs, the entire central system is involved, while in the rabbits it is the bulb which is most affected.

The nature of the histological changes is that of an atrophy of both cell-bodies and fibres, while in some cases the cell-bodies show a granular disintegration and extreme vacuolization. The plates illustrating these changes are very striking and one is led to speculate on how far slight variations in the activity of this gland may initiate in the central system of a normal person, such changes as are here to be seen in an exaggerated form.

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## HEARING.

*Beiträge zur Lehre von der Klangwahrnehmung.* L. HERMANN. Pflüger's Arch., LVI, 10, 11, 12. Pp. 467-499.

*Phonophotographische Mittheilungen.* V. *Die Curven der Consonanten.*

L. HERMANN and FR. MATTHIAS. *Phonophotographische Untersuchungen.* VI. *Nachtrag zur Untersuchung der Vocalcurven.*

L. HERMANN, FR. MATTHIAS and A. EHRHARDT. Pflüger's Archiv., LVIII, 5 and 6. Pp. 255-279.

*A Study of the Sense of Equilibrium in Fishes.* S. LEE. Part II. Journ. of Physiol., Vol. XVII, Nos. 3 and 4.

On the ground of his well-known experiments with the wave-siren, A. König questioned Helmholtz's conclusion that clangs which differ only in the phases of their components are identical for our hearing. Hermann points out that experiments on the wave-siren are themselves questionable, from the fact that on this instrument difference of phase may result in similar sounds from clangs which are fundamentally different in the order of their over-tones. Reversing the curve of a clang as given in a phonograph, either along the axis of abscissas or of ordinate, changes the phases of the components but not the sound.

To the resultant sounds which are already known to arise from a combination of tones, Hermann would add what he calls a *Mittelton*. This tone arises from the actual resultant vibrations impressed on the conducting medium by the components of a clang. In the case of two component tones of the rates of vibration  $m:n$ , the number of vibrations of this 'median tone' within a beat-period would be  $\frac{m+n}{2(m-n)}$ .

From experiments on toothed disks, in which the phase of a tone was rapidly renewed, Hermann concludes that change of phase itself produces tones which may be and probably are the Tarturian tones. The Tarturian tone, therefore, is the intermittent tone from the medial tone. The article concludes with certain hypothetical additions to Helmholtz's resonance theory of tone sensations required to explain intermittent and beat tones. As regards simplicity, however, it is better to assume the direct excitability of the auditory nerve than to add a new series of epicycles to the resonance theory.

In getting consonant curves, Hermann found it necessary to multiply the motion of the phonograph, firstly, by an additional lever, secondly, by a ray of light reflected from the second lever. The motions of this ray of light were photographed. The entire 'plant' of apparatus is extremely complicated and delicate. The present communication gives the curves for  $L$ .

Applying the above apparatus to vowel sounds, Hermann obtained curves five (5) centimeters high. The curves confirmed his views published in former numbers of the *Archiv*, in regard to the fixed form of the characteristic vowel tone.

Dr. Lee's article is in continuation of his study on equilibrium published in Vol. XV. of the *Journal of Physiology*.

The chief find in the present article is that by stimulating the auditory nerve of the common dog-fish (*galmus canis*), the resulting

movements of the eyes and fins is the algebraic sum of the movements which arise from eliminating the ampullae branches separately.

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#### ATTENTION AND MEMORY.

*Recherches sur la mémoire affective.* TH. RIBOT. *Revue Philosophique*, XXXVIII, 376-401.

*Affective Attention.* E. B. TITCHENER. *Philos. Rev.*, III, 429-33.

*Affective Memory.* E. B. TITCHENER. *Philos. Rev.*, IV, 65-76.

M. Ribot attempts to show that pleasures, pains and emotions, as well as olfactory, gustatory and organic sensations generally, are not merely rememberable in an intellectual way as facts that have been experienced, but that they may themselves, in certain cases, be imaginatively reproduced. His conclusion is based on answers to questions received from some sixty persons, a goodly number of whom profess ability to revive, with varying precision and vividness, states of feeling in the manner indicated. Mr. Titchener disputes the interpretation. He does not deny that the recollection of exhilarating sport may be pleasant or that of a whipping painful, that a tooth may be made to ache again by suggestion of the former torment, or that the memory of an insult may excite anger. What he denies in all such cases—and it is on such cases that M. Ribot supports his contention—is that the affection thus aroused is itself a revival of the original affection, and not rather a real affection giving tone to or a real emotion prompted by an ideal object. Seizing on the implied admission that 'revived' affection always attaches to an ideational content, he urges that in that case there is no proof of revival of affection at all; it is a question, not of reproduction versus production, but of production by this stimulus rather than by that. It might be replied that the same is true of every sensational experience that imagination represents. The 'reproduction' is in fact a production, a new reality brought about by the action of a stimulus on a disposition to function, attached by association to elements other than those which attention emphasizes after the recall, and only identical with the original experience as being the same in kind. But the objection lies deeper. The real question, as Mr. Titchener conceives it, is whether pain, pleasure and emotion are possible objects of attention at all. It is because he believes that they are not—a point argued in the earlier article—that he holds that they cannot be singled out and identified in imagination. What can be attended to are the sense-contents. These, however, are condi-

tions, not constituents, of the affectional element, and the denial of our ability to imagine the latter as distinct from really experiencing it as present felt qualification of the represented content appears to the author highly important as a matter of psychological principle.

Certainly, as a matter of pure introspection, it would seem impossible for the attention to fasten on any content corresponding to the abstractions 'pleasure-pain' or 'psychic attitude.' If, as Mr. Titchener maintains, a feeling is 'properly analyzed into sense-substrate and affection,' nothing can be discovered among the objects of direct consciousness corresponding to the latter. On the other hand we can attend, as Mr. Titchener allows, to our concrete feelings. A toothache, a state of grief or terror, can be as distinctly felt as a patch of red color or a movement in the joints. And if felt, then represented as felt, with something, no doubt, of the repercussion of the original excitement. Without this the object represented is not really the same, and the experience is remembered much as a color is remembered which is not visualized; we know, that is, its name, perhaps some of its concomitants. Apart from these experiences, there is nothing in pleasure, pain and emotion for psychology to deal with: they are mere names which express, not psychological experience, but the practical value of the experiences which they qualify.

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#### REACTION-TIME.

*Zur Beurtheilung der zusammengesetzten Reactionen.* W. WUNDT.  
Philos. Stud., X, 485-498. 1894.

*Beobachtungen bei zusammengesetzten Reactionen. Zwei briefliche Mittheilungen an den Herausgeber.* E. KRAEPELIN und JULIUS MERKEL.  
Philos. Stud., X, 499-506. 1894.

*Simple Reactions.* E. B. TITCHENER. *Mind*, N. S., 13, 74-81. Jan., 1895.

*Two Points in Reaction-time Experimentation.* R. WATANABE. *Am. Journ. of Psychol.*, VI, 408-512. June, 1894.

The articles in the *Philosophische Studien* call attention to an aspect of experimental psychology sometimes overlooked—namely, the importance of the knowledge that may be derived from introspection in the course of psychological experiments. Thus Prof. Wundt states explicitly that his theory of the development of the will, and of its relation to 'apperception,' had its origin in observations made during the course of experiments on reaction-time. He

concludes his discussion by saying that the times measured have only an incidental interest—the real value of such experiments lies in the fact that they subject mental processes to fixed conditions, and thus make possible an exact analysis by introspection. While much can be said for this point of view, the present articles do not give conclusive testimony in its favor, as they are controversial, the introspective evidence of some observers contradicting that of others.

Wundt argues for the interpretation of sensory and motor reactions, 'perception-times,' 'discrimination-times,' 'choice-times,' and 'association-times' already given in detail in the fourth edition of the *Physiologische Psychologie*. Wundt is regarded as the great representative of the experimental and scientific method in psychology—and deservedly so—but he does not readily adapt himself to the scientific attitude that weighs evidence and waits for evidence. He considers it possible and desirable to pass final judgment on every question great and small. This he does with much learning and ability, but often without proper perspective. He sees the world as a panorama with himself in the centre. He forgets that a panorama, constructed from fragmentary data, holds only for the individual who constructs it—also that there is no centre of infinite space.

The experiments on sensory and motor reactions do not seem to the present writer nearly so important as they do to Wundt, nor can he admit Wundt's interpretation of the facts. When Wundt informs us that "zu Versuchen über den zeitlichen Verlauf psychischer Vorgänge ist nun von vornherein nur ein Beobachter fähig, der im stande ist, willkürlich zwischen diesen beiden Reactionsformen zu wechseln," he is proposing an esoteric psychology, not a scientific method. Wundt insists that the 'subject' in psychological measurements must always be a skilled psychologist. Yet he writes on *Thierseele*! The investigator should, indeed, be a skilled psychologist, able to interpret the facts, but a psychologist with a theory to prove is not a good observer.

Every one who wishes to make psycho-physical time-measurements should read the article by Wundt and the letters in the same number of the *Studien* by Prof. Kraepelin and Dr. Merkel—not in order to accept as a matter of course the observations given—but in order to realize the need of observing and recording the changes in consciousness accompanying such experiments.

Prof. Titchener's discussion of sensory and motor reactions in *Mind* is more careful and judicial than is Wundt's. He sums up the

evidence of ten researches and finds six favorable to the distinction and four more or less negative. Prof. Baldwin, however, seems to be counted on the wrong side, as he finds (in a publication later than the one quoted) the nature of the difference to vary with the observer; and Prof. Titchener himself has found the distinction in less than half the cases he has tested. We may conclude that the normal reaction-time of an observer can often be lengthened by directing him to fix his attention on the sense-impression, but it does not seem so evident that it can be shortened by directing him to fix his attention on the movement. The reaction-time is naturally lengthened and made more irregular when its automatic nature is disturbed; and from the experiments made in the Leipzig laboratory, it would seem that attending exclusively to the sense-impression is more disturbing than attending exclusively to the movement. In daily life, however, the contrary holds; actions are executed more automatically when the attention is directed to the sense-impression—thus in throwing, catching or striking a ball, the more completely one can attend to the ball and forget the movement, the more efficient and quick is the movement. Indeed, in reaction-time experiments, when the stimulus is so strong as to compel the attention (as with painful electric shocks), the reaction-time is very short, which would seem conclusive against the extreme views of Lange and Wundt. That the difference between the times of sensory and motor reactions gives the time required to perceive the stimulus (Wundt and also Titchener in his earlier paper, *Philos. Stud.*, VIII.), does not seem admissible to the present writer.

In the short paper by Prof. Titchener and Mr. Watanabe, attention is again called to the desirability of treating reaction-time experiments from the point of view of *psychology*. The observer's impression regarding the nature of the reaction is recorded. The writers conclude that in the case of sensory reactions introspection affords an adequate control, but is less trustworthy in the case of muscular reactions.

J. MCK. C.

#### JUDGMENT AND BELIEF.

*Glaube und Urtheil.* W. JERUSALEM. Vierteljahrschrift für wissenschaftliche Philosophie. Vol. XVII, pp. 162-195.

*Grundzüge der Logik.* T. LIPPS. Hamburg u. Leipzig: Voss.

*Principii di Logica Reale.* N. R. D'ALFONSO. Rome: 1894.

*Appearance and Reality (passim).* F. H. BRADLEY.

*The Test of Belief.* J. P. GORDY. Philosophical Review, May, 1894, 257.

Few states of consciousness, or psychoses, whether viewed from the psychological or from the epistemological standpoint, are

more interesting or more important than those indicated by the words 'judgment' and 'belief.' If it be true that the whole essence of the thinking process is involved in the formation and expression of judgments; that judgment is not so much a mere occurrence in the mind as an activity of the mind; that the test of a genuine act of judgment is the presence in it of belief; and that in all judgment there is thus a 'trans-psychosial' reference, a reference, that is, to reality beyond the factual sphere of the psychosis as such:—if all this and much more that we are told of judgment and belief, be true, then it would scarcely be too much to say that a good means of testing the psychological and even the epistemological position of any writer would be to ask "What is his doctrine of 'belief' and of 'judgment'?" We summarize a few recent utterances bearing upon these subjects:—

Herr Jerusalem calls attention to the fact that in recent times the view has often found expression that the essential characteristic of the act of judgment is the consciousness of its objective validity, called by the English *belief* and by some German psychologist *Anerkennung*. This view has been urged especially by J. S. Mill (Notes on Jas. Mill's *Analysis*, I, p. 342, and *Exam. of Ham. Philos.*, p. 405) and by Brentano. (*Psychol. vom empir. Standpunkte*, I, pp. 269 f.). Attention is also called to the important discussions of belief by James (II, 282 ff) and Baldwin (II, ch. 7 and *Mind*, N. S., I, 403); the last named has handled in a very noteworthy manner the question of the relation of belief, feeling and judgment. The trans-psychosial reference inherent in every judgment and characterizing it as something more than a mere psychosis, a mere affection of consciousness, was recognized even by the ancients, e. g., by Plato (*Theat.*, 184-187) and the Stoics (Cicero, *De Fato*, 19, 43). Descartes and Spinoza emphasized the presence of a conative element in judgment and in belief; with them judgment is predominantly an assent of the will, an affirmation. The history of the problem of judgment shows that it has been handled either in a one-sidedly *psychological* or in a one-sidedly *grammatico-logical* manner. Baldwin has rightly insisted that a complete theory of judgment can only be attained when all the constituent factors, or elements, entering into the act are given full recognition.

Herr Jerusalem thinks that the whole subject of judgment needs to be investigated anew, and especially does the relation of belief to judgment need to be made clear.

Judgment is, he finds, an activity by which the complex of sensation, or manifold of sense, is discriminated and combined, moulded

and articulated, and objectified, *i. e.*, regarded as an independent unitary being with powers. Consciousness in judging conceives the given manifold or complex as the activity of a thing. Judgment is essentially 'ein Gliedern und Gestalten.' An injection of an element of willing into the presentative complex is the most important factor in an act of judgment. In fact, in judgment the given content of sense—presentative content—is formed or moulded by a process analogous to the activities of our own will, and objectified or conceived as an activity or quality of a thing. In this objectification we find the germs out of which belief and the conception of truth later develop. This objectification being present implicitly in sense-perception, we may say that even in perceiving we judge.

What now is the relation of judgment to truth? Truth, as already said, is implicit in the objective reference characteristic of all judgment. Mill is right in saying that to judge and to regard the judgment as true are identical. This, at any rate, is true of original and naïve judgments. The full consciousness of truth is, however, only reached when by experience we are taught the possibility of error. The truth of a judgment is the relation between the judgment as a psychological fact and the judged event. We denote this relation by word the 'accordance' (*Entsprechen*). The idea of truth first arises by reflecting on this relation. Such reflection, however, only becomes possible when we discover that wrong interpretations, mistakes, occur. In defending the meaning contained in a judgment against possible assaults the consciousness of truth emerges. The conception, therefore, of truth presupposes experience of error. Truth and error both belong properly to the sphere of judgment. Bradley's distinction between an 'idea as a fact' and an 'idea as a meaning,' more properly holds of judgments than of concepts. We can, that is, distinguish between a judgment as state of consciousness and a judgment as having a 'meaning'; and truth is the relation of these two sides of the judgment to each other. Indeed only in a system recognizing a world of extra-mental realities, independent of judgments and to which they may conform or not, is truth possible; that is, truth presupposes psychoses and a trans-psychosial world of realities; deny either and the merely factual, not truth, is all that is left. The criteria of truth are found in the fulfilment of predictions and the agreement with other thinkers.

What now is the nature of belief, and what is its relation to judgment? An element of belief is implicit in the act of judgment; but this embryonic belief is to be carefully distinguished from belief in the higher sense. Belief as a clearly experienced state of con-

sciousness is the holding as true of a judgment and therefore it presupposes judgment and the concept of truth. Yet the truth of a judgment is in no way a condition of belief; untrue judgments are believed as well as true ones. The English psychologists are right in finding in *feeling* the source and essence, psychologically, of belief. The opposite of belief is not disbelief but doubt, which is generally and rightly regarded as feeling; belief, therefore, is predominantly feeling. 'Predominantly,' for all psychic facts—all really experienced psychoses—consist, without exception, of more than a single factor, comprise always intellectual, conative, and affective elements. They are named and classified according to the predominant factor, and in the case of belief, this is *feeling*. Belief, as here used, is not to be contrasted with knowledge; it is used in the general sense of 'holding as true.' What calls forth this feeling which attaches itself to a judgment and turns it into one held as true? The answer is, that belief is the *feeling of harmony*, or agreement with the previous content of my consciousness; the feeling of the accordance of a judgment with my conceptions of the world. Just as doubt arises from the conflict of a judgment with my previous thoughts, so the feeling of belief springs from their harmony.

Herr Jerusalem's paper is a very meritorious one and will repay study.

Prof. Lipps also emphasizes the objective, or 'trans-psychosial,' reference in all judgment. Judgment is the consciousness of the objective necessity of a relation, or union, of the objects of consciousness. The logical doctrine that judgment states only what is true or false, is sound. Truth is synonymous with real knowledge. The distinction is made—fundamental for logic—between real and formal judgments. In a formal judgment the objective necessity is an unconditional necessity prevailing among notions; in a material judgment the objective necessity is that of relating, to an object of consciousness thought as objectively real and so far as it is thus thought, another also thought as objectively real. The objectively valid judgment is the special act of real knowledge. A judgment is objectively valid when the consciousness of the objective necessity perdures, without contradiction, against all possible experience and objectively necessary union of the objects of experience. Objectively valid judgments, hence knowledge, arise in the struggle and interaction of the proximate subjectively valid judgments. Every judgment is subjectively valid in so far as it is made.

The universal validity, or validity for all, follows from the objective validity, on the assumption of similarity in the thinking

processes of all thinking beings. That is, the claim to universal validity of a judgment lies in the conviction, that, on account of the similarity of all minds, all must reach like judgments, in so far as they have the same experiences and relate them by thought. Prof. Lipps' discussion is, from the logical standpoint, singularly fresh and helpful.

Signor D'Alfonso in his little treatise on concrete logic has some remarks of interest on judgment (*considerazioni sul giudizio*). All thinking and reasoning are essentially judging; in judgment is involved the whole of the thinking process. Every judgment implies in one and the same act a synthesis and an analysis; these are the two sides of every judgment. Every so-called negative judgment can be transformed into a positive one. When we assert that a given body is not solid we implicitly assert that it is liquid or gaseous. Negation is, it would seem therefore, a judgment on a judgment and thus presupposes an affirmative judgment. Psychologically affirmation is prior to negation—in fact all judgments are, psychologically, affirmative. That is, as concrete mental processes there is no distinction between positive and negative judgments; the attitude of mind in up and down negation being the same as in affirmation. It is, therefore, the *non-licet* attitude of mind, the refusal to (logically) affirm or deny, which is psychologically the opposite of judgment.

Mr. Bradley is more interested in the epistemological and metaphysical aspects of judgment and belief than in the purely psychological. But his book is full of keen psychological analyses and deserves, as was made evident by Prof. Baldwin in a late number of this REVIEW, the attention of students of psychology. Those acquainted with Mr. Bradley's *Principles of Logic*, will not be surprised to find that in the more recent work he has a good deal to say of judgment. We extract a few pregnant statements: In judgment, according to Mr. Bradley, we find thought in its completed form. Judgment is the differentiation of a complex whole, and hence always is analysis and synthesis in one. It separates an element from, and restores it to, the concrete basis. And here obviously the synthesis effected is a re-union of the distinguished, and implies the separation, which, though it is over-ridden, is never unmade. The predicate is a content which has been made loose from its own immediate existence and is used in divorce from that unity.

In every judgment there is in the subject an aspect of existence which is absent from the bare predicate. No one ever *means* to assert about anything but reality, or to do anything but qualify a

'that' by a 'what.' Judgment adds an adjective to reality. In every judgment the genuine subject is reality, which goes beyond the predicate and of which the predicate is an adjective. The predicate, on the other hand, is a mere 'what,' a mere feature of content, which is used to qualify the 'that' of the subject. In every judgment, then, we find an aspect of existence, absent from the predicate but present in the subject, and in the synthesis of these aspects we have got the essence of judgment.

Prof. Gordy's paper is a fitting companion-piece to Herr Jerusalem's. It is so full of matter that it is difficult to condense it and yet do it justice.

The distinction between the pure intellect seeing and the practical intellect trusting, or between knowledge and belief, Prof. Gordy considers of fundamental importance. Belief of any kind consists, he declares, of two factors: what, with Baldwin, we may call the reality-feeling, plus the 'consciousness of the personal indorsement of reality.' One of these elements or constituents of belief—the reality-feeling—we may have without the other—the personal indorsement of the reality; the saying to one's self that the reality-feeling is true. "Sitting in a car at a depot, waiting for my train to start, I seem to see the motion of my train when another train moves slowly by. In other words, the reality feeling attaches itself to the image or idea of my train in motion. But when I look at the wheels of the moving train this reality feeling ceases to exist so long as I continue to look at them. I see or believe that the apparent motion of my train is due to the real motion of the other. The same kind of reality-feeling attaches itself to a new set of experiences. But as soon as I stop looking at the wheels, the old reality-feeling returns—my train seems to move in spite of the fact that I know it does not. In other words, the reality-feeling, which alone distinguishes the ideas or images of memory from mere imagination, attaches itself to experiences which we know from other evidence do not represent reality." For further discussion of this point we are referred to 'Baldwin's able and very lucid treatment of the subject,' *Feeling and Will*, ch. 7.

By a critical examination, containing much that is suggestive, of Prof. Bain's three postulates or assumptions underlying all material or inductive inferences, and of J. S. Mill's theory of induction, Prof. Gordy reaches the conclusion that in order to carry on the reasonings of ordinary life as well as those of science, we must assume (1) the trustworthiness of memory within certain limits, (2) the uniformity of nature, and (3) that an hypothesis that explains a particular group of facts, and at the same time harmonizes with the rest of our

beliefs, is true. We can give no reasons for such beliefs which would at all satisfy a cold, critical intellect, an intellect indifferent to consequences, an intellect that believes only in so far as it sees grounds for certainty or for probability. Now from the point of view of the pure intellect, the intellect *seeing*, not trusting, these beliefs have neither certainty nor probability; from the point of view of the practical intellect, the intellect yielding to the native instincts and unreasoned tendencies of the mind, they are not only probable but certain. From the point of view of *knowledge*, in a word, our beliefs are so many pure assumptions.

Now we need a test of belief. By 'test of belief,' Prof. Gordy does not mean a test by means of which we can determine the truth of our beliefs, that would be a test of truth. He means a formulation of the marks or characteristics of the beliefs that we are obliged to assume without proof. Now we can say that, since we have accepted the trustworthiness of memory and the uniformity of nature and the proposition, 'an hypothesis that explains facts, and at the same time fits in with everything else that we believe is true,' we will accept any other proposition without further proof that has the same characteristics. What, then, are the characteristics of these beliefs? The assumption of the trustworthiness of memory has two: (1) it is a belief that we have a natural tendency to make,—*i. e.*, when we begin to reflect we find ourselves making it;—and (2) experience does not deprive us of it. The second characteristic—the confirmation of experience—must be taken in a negative sense only. Of positive verification of the trustworthiness of memory, we have none. The thesis which Prof. Gordy maintains, then, with reference to the trustworthiness of memory is this: What we know on the authority of what we call memory has no other guarantee than a reality-feeling, a feeling which sometimes attaches itself to experiences that we know do not represent realities, but which we accept in the case of memory, simply because it is *not contradicted by other experiences*.

The characteristics of our belief in the uniformity of nature are the same; we have a natural tendency to make it, and our experience is not inconsistent with it. What again, are the characteristics of the third assumption: An hypothesis is true that explains the facts, and that takes its place easily and naturally among our other beliefs. They are the same. These, then, are the characteristics of the three assumptions (beliefs), one of which underlies all reasoning whatever, and all of which underlie the reasoning of inductive science and everyday life.

Necessary truth, then, aside—truth, that is, whose contradictories are 'absurd, inconceivable, impossible,'—whatever we are asked to believe, ought to be *either* an ultimate belief, *i. e.*, a belief having the characteristics of being assumed through a natural tendency, and of not being interfered with by experience, *or* an hypothesis that explains all the pertinent facts, and takes its place easily and naturally among our other beliefs. The broader the base of experience upon which beliefs, in the negative sense explained, rest, the greater their credibility. If one man accepts one hypothesis because it explains all the facts he knows, and another man a different hypothesis because it explains, not only the facts known to the first man, but others equally certain, the last man's hypothesis is the more credible, although we can never say that it, in turn, may not have to give place to another.

Such a theory, it may be urged, opens the door to unbounded credulity. Not so, says Prof. Gordy, for the very prominence which it gives to the fact that inductive reasoning is only a process of finding hypotheses to explain facts, cannot but enforce the necessity of caution on the part of one who accepts it. Again, it may be urged, that its practical outcome is philosophical skepticism. Not so, for he only can be charged with philosophical skepticism who holds that reason is hopelessly at war with itself; who holds that, no matter upon what subject or in what direction he tries his reason, it leads him into an inextricable tangle of inconsistencies and contradictions. With the common-sense philosophy, the theory insists that the attempt of the empiricist to find positive verification in experience for the first principles of science cannot succeed; with empiricism, it insists that the attempt of the common-sense philosophy to establish definite philosophical principles must end in failure. Finally, the theory aims to give full recognition to the important, nay, the decisive, part which the emotional and volitional side of our natures play in shaping our beliefs.

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#### PATHOLOGICAL.

*Psychiatrie.* TH. ZIEHEN. Berlin, Friedrich Wreden, 1894. Pp. 470.

The author has already made several contributions to physiological psychology and the present text-book on psychiatry is frankly written on psychological lines, as distinguished from clinical. Ziehen claims that the association psychology is entirely sufficient to explain all the facts of psychiatry and over one third of the book is given up

to general psychology, and he discusses the disturbances of sensation, of ideation and memory, of the intellectual feelings, of the association of ideas, of behaviour, and the accompanying somatic symptoms of the psychoses. The psychology is orthodox, albeit somewhat dry, but is on the whole satisfactory, and furnishes a good compendium of the perverted mental operations of mental disease. In his classification Ziehen makes but two grand subdivisions, psychoses without defect of intelligence and those with such defect. Under the first coming the simple psychoses, mania, melancholia, neurasthenia, stupor and paranoia, and the combined psychoses—the insanities secondary to the above. Of the psychoses with defect of intelligence there are first the states of congenital defect, idiocy, imbecility and debility, and secondly the psychoses from acquired defects, the six forms of dementia, paralytic, senile, secondary after brain lesions, secondary after functional psychoses, epileptic and alcoholic. In this simplification of classification there are several important omissions. *Delirium acutum* is denied a place as a distinct clinical entity, against the opinion of the best alienists, and is only spoken of as occurring in acute hallucinatory paranoia and in general paralysis. Again, periodical and circular insanity are simply assigned places as varieties of mania and melancholia, and under the degenerative psychoses. But it is in the field of Paranoia that recent German writers have especially run riot, and Ziehen adds greatly to the already existing confusion. The moment we depart from Krafft-Ebing's definition that paranoia is a chronic disease, showing itself exclusively in degenerate individuals, and frequently developing from the constitutional neuroses, and whose chief symptoms are [systematized] delusions,—we are landed in inextricable confusion. Ziehen's acute hallucinatory paranoia terminating in recovery without mental defect in over 70 per cent. of all cases, is not 'Verrücktheit,' and bracketing them together only tends to confusion and false ideas. Ziehen makes four forms of paranoia, the acute and chronic hallucinatory, and the acute and chronic simple paranoia. In his etiological summary of the psychoses the following etiological factors are credited with producing, besides many other psychoses, different forms of paranoia, as follows: Hereditary degeneration, three forms; trauma capitis, two forms; chronic alcoholism, five forms; puberty, three forms; senility, two forms; climacteric, two forms; puerperal state, two forms; lactation, two forms, acute febrile diseases, three forms [in reality confusional insanity]; epilepsy, two forms; hysteria, four forms; exhaustion, four forms. It would be hard to conceive of confusion worse confounded. Ziehen's simple chronic paranoia is the only one that fulfils the condition of a

chronic primary disease with systematized delusions. When it is considered that Jean Jacques Rousseau, Ludwig of Bavaria and Guiteau are classical examples of paranoia in its proper and more restricted sense one sees the folly of speaking of a man suffering from a blow on the head, or from pneumonia or multiple neuritis, where mental disturbance develops, as being a paranoiac.

Ziehen adopts the sound modern doctrine that in the immense majority of cases gynaecological treatment is entirely without influence, while in other cases by setting up new irritations it is positively harmful.

Katatonica is admitted to a position as a clinical entity, but is given as of rare occurrence.

The clinical descriptions are clear, and the ten photographs are remarkably successful in giving the physiognomy of the different diseases.

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WILLIAM NOYES.

*Les états intellectuels dans la mélancolie.* G. DUMAS. Paris, Alcan, 1895.

This little volume is worthy of more than casual notice. M. Dumas, a pupil of Ribot, belongs to the group of contemporary psychologists who commenced their study with philosophy and metaphysics, and then changing face, have gone over into medicine. MM. Janet and Marillier did the same.

This study on melancholy is Dumas' thesis in medicine. One can not praise too highly the courage of those who, while already having degrees and titles as doctors of literature and professors in schools and colleges, yet devote themselves to undergraduate work in medicine. Their intellectual experience is peculiar. Instead of commencing the study of psychology with observation and fact, they have approached it from the side of the more abstract and metaphysical problems. They seem to put the chariot before the horse; and it becomes an interesting question what attitude this leads them to bring to the empirical study of medicine. Do they still remain metaphysicians?

Dumas' study relates to four women of the asylums of St. Anne and Salpêtrière in Paris, all afflicted with melancholy. He studies their mental state with their physical symptoms. He distinguishes four forms of the trouble: Melancholy with stupor, 'anxious' melancholy, 'depressive,' and 'conscious' melancholy. The author occupies himself mainly with the last two kinds.

His main method of study was by conversation with the patients, seeking to gain their confidence, questioning them of their griefs,

endeavoring to reason with them and to reassure them. Evidently this is a more fruitful method than direct experiment since the field of mental disturbance is so wide. Yet the author felt the need of more than this bird's-eye view of his patients' mental state and sought to study more exactly the rapidity of perception and memory, the naming of objects, and the localization of sounds, &c. His arrangements for this were a little inexact; for without careful arrangements for measuring intervals of fractions of a second no definite results can be obtained. The same may be said of the methods of studying physical symptoms: the author is satisfied with stating *what he saw*: such as attitudes, changes of cordiac pressure (from 800 gr. to 500 gr., on a Verdin sphygmometer, &c. He himself says that he might have given his results more exactness by plotting curves and giving tracings. Yet he concludes: "I am convinced, after many efforts, that the methods of psychophysics are not applicable to phenomena so complex as those I wish to study" (p. 142). But would it not have been better to publish his figures and tracings and then to show by a critical discussion of them why these exact methods are not applicable to these patients?

In the opinion of the author the ground of melancholy is not emotion, a psychological entity, but an organic state, a depression of the organism, a lack of nutrition. It is a predominant activity of the organs which produce the particular sensations contributing to the mental state of sadness, anxiety, depression. In this view he discusses the theories of James and Lange with just criticism. He explains from this point of view the infectious character of melancholy, citing the general vital depression which follows an attack of influenza. "It may be objected," says he, "that organic depression is produced some times as a consequence of mental trouble; the fact can not be denied, but it is far less frequent than we think." And he cites instances of the contrary (p. 100).

We need not say that the author is right here, only we should advise him, if he would go deeper, to make his distinctions more exact, *i. e.*, to show how physiological depression lies at the basis of melancholy. It seems probably true; but depression is a very vague word and the author finds depression present in a variety of diseases whose emotional tone is very different from one another, *i. e.*, hysteria, dyspepsia, heart troubles, &c. Has not the particular organ affected in each case, some special importance? Would it not be interesting to enquire into the particular organic derangement which is found in each of these troubles?

Although melancholy is, in the opinion of the author, consciousness of a state of the body, yet in certain instances it may

arise from intellectual conditions ; it may take its origin in an event which distresses, depresses, and finally enfeebles the patient. So also melancholy which arises from an organic cause is always aggravated by the distress which results from it, so that the psychological phenomenon, be it cause or effect, always plays an important part in the development of the disease. There is a series of complex actions and reactions between the physical and the mental.

The second question which the author studies is the nature of the intellectual changes which take place in melancholy. The principle features of this mental state, according to his short and summary descriptions, are 1. The slowing of the mental flow and great mental impoverishment ; 2. Aboulia, or the incapacity to carry out an act conceived. Of this the author cites two examples. One of his patients made careful preparations for suicide, but lacked courage at the critical moment ; another wished to write a letter but desisted from scruples of doubt. He explains it as a defect of idio-motor synthesis ; 3 The development of automatic acts sometimes very grave. One patient suddenly attempted suicide and had great trouble in recognizing herself as the perpetrator. This proves the act automatic. Indeed she thought the command came from a foreign will. In this connection the author studies the melancholy of Hamlet in whom he finds the signs which he thinks characteristic of melancholy ; 4. The last mental sign of melancholy and the most important is the lack of logic: a patient weeps from organic causes merely and without knowing why, or when the necessity arises of thinking of some old distress long since forgotten in the past. She nurses the thought of these miseries and comes to believe that they cause the present grief.

In conclusion we may hope that the author will continue and deepen his study of these questions and give us more extended observations in detail. As it is his little book is clear and attractive.

*Cliniques des maladies du système nerveux.* J. M. CHARCOT. Tome II. Paris, Alcan, 1893. Pp. 482.

For some time M. Charcot, like the true teacher that he was, associated his students with all his labors, and even allowed them to take his place in some of his clinical lectures, to give them opportunities to explain, to all those who frequented the Salpêtrière, the questions studied by him. It is in this way that after preparing a study on great calculators, in co-operation with M. Charcot, I was requested by him to deliver a lecture at the Salpêtrière on memory for numbers. The present volume, prepared some months after the

death of the eminent professor, has been written with the co-operation of a great number of his pupils; only five or six of M. Charcot's own lectures are included in the volume. They bear on subjects that do not all equally concern psychology; but psychologists may read with profit the lecture on hysterical hemianaesthesia and toxic anaesthesia (p. 460), and with still more profit the one on retro-antegrade amnesia. Let us recall in a few words what this anaesthesia consists in. The question is of a patient who after a nervous shock and crisis had retrograde amnesia, and also because incapable of registering actual facts in memory. M. Charcot shows clearly that this amnesia is not real but apparent. The patient remembers very well the facts which she seems to forget, because she talks in her sleep of facts of which she has no idea in her waking state; and moreover in the hypnotic state she remembers all the incidents of the whole period since the nervous shock. (p. 266). This volume also contains equally interesting studies by Guinon and Blocq on states of somnambulism.

SORBONNE, PARIS.

A. B.

#### NEW BOOKS.

- The Philosophy of Mind.* G. T. LADD. New York, Scribners, 1895.  
Pp. XIV + 414. \$3.
- Elements of Ethics.* J. H. HYSLOP. New York, Scribners, 1895.  
Pp. VII + 470. \$2.50
- Monism as Connecting Religion and Science.* E. HAECKEL. London, Black; New York, Macmillan, 1894. Pp. VIII + 117.
- The Factors in Organic Evolution.* D. S. JORDON. Boston, Ginn & Co., 1894. Pp. V + 149.
- Comte, Mill and Spencer: An Outline of Philosophy.* J. WATSON. Glasgow, Maclehose; New York, Macmillan, 1895. Pp. XX + 302. \$1.75.
- Mental Development in the Child and the Race: Methods and Processes.* J. M. BALDWIN. New York and London, Macmillan, 1895. Pp. XVII + 496.
- Amphioxus and the Ancestry of the Vertebrates.* A. WILLEY. Columbia University Biological Series, II. New York and London, Macmillan, 1894. Pp. XIV + 316.
- Transactions of the Illinois Society for Child Study.* Vol. I, No. 1. Chicago and New York, Werner Co., 1895. Pp. 73 + XLIII. 50 cts.
- Imagination in Dreams and their Study.* F. GREENWOOD. London, John Lane; New York, Macmillan, 1894. Pp. IX + 198.

- The Study of Ethics. A Syllabus.* J. DEWEY. Ann Arbor, Register Publishing Co., 1894. Pp. 151.
- Elements of Psychology. Syllabus of Philosophy, I.* J. H. HYSLOP. New York, Columbia College, 1895. Pp. 131. \$1.
- Popular Scientific Lectures.* E. MACK. Translated by T. J. McCormack. Chicago, Open Court Co., 1895. Pp. 313. \$1.
- The Psychology of Childhood.* F. TRACY. Second ed. Boston, Heath, 1894. Pp. XIII + 170.
- Twelfth Annual Report of the Bureau of Ethnology (1890-'91.)* J. W. POWELL. Washington, Gov. Printing Office, 1894. Pp. XLVIII + 742.
- Logic.* C. SIGWART. Trans. from second German edition by H. Dendy; 2 vols. London, Sonnenschein; New York, Macmillan, 1895. Pp. XII + 391 and VIII + 584. \$5.50.
- Lehrbuch der Psychologie.* W. VOLKMANN. Edited by C. S. Cornelius. Fourth ed., Bd. II. Cöthen, Schulze, 1895. Pp. V + 568.

## PSYCHOLOGICAL QUESTIONING.

### ON OUR EARLIEST RECOLLECTIONS OF CHILDHOOD.

We possess very few observations on our earliest recollections. I should like to make a series of observations in this subject. I shall be grateful to all persons who will send answers to any or all of the following questions :

1. Age and usual occupation.
2. Do you have good *visual* representations of object in general ; viz., can you form a visual image of an apple or of a lamp, etc. ?
3. Do you have good *auditory* representations (of sounds), viz., have you auditory representations of the voices of your friends ?
4. What is the earliest recollection of your childhood ? Please describe it as fully as possible. How clear is it, and what was your age when the fact recollected occurred ?
5. Had this fact a particular importance in your life, and if so, in what way ?
6. Has anyone ever related this fact to you, or do you remember it yourself ?
7. Can you give any explanation of this recollection, and if so, what ?
8. What is the second recollection of your childhood ? How far apart are these two in time ?

9. Of what period of your life do you first have many recollections without connecting them in the time series of your life? How do they appear; are they clear, are they visual or auditory, etc.?

10. From what period of your life do you begin to have recollections of the time series of your life?

11. Do you ever have recollections of your childhood in your dreams? If so, what?

Please send the answers to these questions to Victor Henri, Leipzig (Germany), Johannis Allée 12. II.

#### NOTES.

Mr. J. S. MacKensie, M.A., has been called to the chair in Philosophy in University College, Cardiff.

The *Année psychologique*, of which announcement was made in an earlier number of this REVIEW, will be issued in March, 1895. The subscription price (7 fr., instead of 5 fr., as previously announced) may be sent directly to M. Alf. Binet, 29 Rue Madame, Paris, France.

We have received Bd. I, Heft 1, of a new serial publication, edited by Prof. E. Kräpelin, of the University of Heidelberg, entitled *Psychologische Arbeiten* (Leipzig, Engelmann, 5 M.)

The attention of readers of the REVIEW is called to the special announcements made by the editors on the second cover-page of this number.

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